

Psychological Review

EDITED BY

CARROLL C. PRATT
PRINCETON UNIVERSITY

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THE PSYCHOLOGICAL REVIEW

GRACE MAXWELL FERNALD¹

1879-1950

Grace Maxwell Fernald was born in Clyde, Ohio, on November 29, 1879. She died at Los Angeles, California, on January 16, 1950, at the age of seventy years. The life and accomplishments of this good, gracious, and brilliant woman are of such a nature and importance that she will be remembered and valued for many years to come by the innumerable students whom she instructed and the many persons she helped. Her contributions to theories and methods of learning directly influenced many lives and will continue as an educational force in future generations.

Grace Fernald was a member of a most distinguished and scholarly family. She was the daughter of James C. Fernald and Nettie Barker Fernald. Her father was the author of many notable books on English including *Students' Standard Dictionary*, *Synonyms and Antonyms*, *English Grammar*, *Historic English*, *Expressive English* and many others that are reprinted and in use at the present time, more than thirty years after his death. Grace's mother, Nettie B. Fernald, was a most unusual woman. Her personal charm and staunch character were important influences in the lives of her children and in the commu-

nity in which she lived. Grace had one sister, Mabel R. Fernald, also a psychologist of first rank. Her four brothers, Charles, Henry, Dana and James, have made an outstanding record in their various fields of endeavor.

Grace's early life was spent with her family in New York and New Jersey. The family roots were laid in Ohio. After graduation from high school, Grace continued her education at Mt. Holyoke, Bryn Mawr College, and at the University of Chicago. She received her A.B. in 1903 and her M.A. in 1905 from Mt. Holyoke. In 1907 the Ph.D. was conferred on her by the University of Chicago in the field of Psychology. She liked to add, "*working under the direction of James Rowland Angell.*" She had an assistantship and a scholarship at Bryn Mawr and a fellowship at the University of Chicago. She returned to Bryn Mawr as Instructor in Psychology in 1907. Later she was appointed as the first psychologist to work in the Juvenile Court in Chicago, and was associated in that work with Dr. William Healy. In this position in the Juvenile Psychopathic Institute, which under another name has continued as a research and service center in this field, she made significant contributions in the field of juvenile delinquency and in the development of performance tests of intelligence.

¹ A minute prepared for the Academic Senate of the University of California by Ellen B. Sullivan, Roy M. Dorcus, Bennet M. Allen, and Louis K. Koontz.

Grace Fernald came to Los Angeles in 1911 to join the faculty and head the Psychological Department and Laboratory at the State Normal School. She continued in the Normal School and in the University of California at Los Angeles from that time until her death. She had the title of Assistant Professor, Associate Professor, and Professor in these thirty-nine years of fine service. She was loved, admired, and appreciated as the advisor and the inspiration for thousands of students who took her courses and of the faculty and friends who were associated with her. She was a leader in moves for the betterment of schools, corrective and penal institutions, and for changes for the improvement of general civic conditions. In this connection she had many appointments of honor from the governor and city and state officials.

Grace Fernald was a member of top rank in all of the professional and scientific organizations related to her field of work. She was a Fellow of the American Psychological Association, the American Association for the Advancement of Science, the American Association of Applied Psychology and, from its beginning, held the Diploma in Clinical Psychology awarded by the American Board of Examiners in Professional Psychology. She had earlier been elected to membership in Phi Beta Kappa, Sigma Xi, and Pi Gamma Mu.

Dr. Fernald was the author of many articles in scientific journals in the fields of experimental, theoretical, child, educational, clinical and mental measurement psychology. Her first publication, her Ph.D. thesis, was an outstanding contribution ("After sensations from subliminal stimuli in peripheral vision"). Monographs on child delinquency and mental tests were followed by her significant work on remedial methods and on the adjustment of bright students with learning difficulties

in special areas of reading, spelling, composition, foreign languages, mathematics, and speech. Her last major publication, *Remedial Techniques in Basic School Subjects*, continues to exert a profound influence in corrective education as well as in the field of prevention of maladjustment. This book was published in 1943 and has run through many reprintings to date. It has been a problem to supply enough copies to satisfy the continuing demand.

In addition to a teaching career of breadth and depth, a professional level of accomplishment in her field that is unusual, and authorship of many publications of note, Grace Fernald developed and directed the Clinical School in the University of California at Los Angeles. From 1921 until her death this School has been an unusual and unique institution in the field of education and psychology in universities. The School has served as a laboratory for research in clinical problems and methods. The children and adults as well as the teachers and psychologists who were helped by study and treatment in this clinic run into the thousands. Plans are in progress to assure the continuation and development of the Clinical School as a memorial to Dr. Fernald, to meet the obligations of the University to persons who need help, and to provide for the advancement of research and instruction in this field.

Among many activities of community as well as scientific value carried on by Grace Fernald through the years, a few might be mentioned: She wrote a series of textbooks and a teacher's manual on *Spelling* for the California State Board of Education which were used for many years in the schools of California. She collaborated with August Vollmer and others, appointed by the Governor, in a survey of recreational activities in the institutions and communities of California, resulting in a publication of find-

ings and in an improvement in this situation. She did much to establish psychological services in schools and institutions in California. Upon request she acted as a consultant and planned and directed remedial work for an Army Camp in California dealing with illiterate soldiers drafted into military service in World War II. These and many other activities had her interest and support throughout the years.

After Dr. Fernald's retirement from active service at the University, she continued as an advisor and consultant in the University Clinical School. At this time she established her own private Clinic and Clinical School in Brentwood, and was successfully active in this work until her death in January of this year.

Grace Fernald was a warm and vivid person, completely unselfish and objective. She loved children and animals and all living creatures. She felt a personal obligation to look after and serve them in their troubles no matter how inconvenient it might be. She traveled widely when she could escape the gruelling schedule she set for herself. She was welcomed and sought after whenever she came and wherever she went. We have been privileged to know Grace Fernald. We shall sorely miss her. She slipped away suddenly, but quietly and peacefully. Her published works will be a lasting memorial and her ideas will be carried out by all whom she taught and helped to a better adjustment.

CONSCIOUSNESS AND THE GALVANOMETER

BY HAROLD GRIER McCURDY

University of North Carolina

It is my aim in this paper to rescue from neglect the genuine and dependable relationship obtaining between the psychogalvanic reflex, called by a recent author "probably the best general index of amount of total neuromuscular activity that we have today" (5, p. 13), and consciousness, which this same writer like many others wishes to relegate to the scrap-heap.

From the earliest period of PGR experimentation it has been asserted that magnitude of galvanometric deflection is intimately associated with the subject's estimate of the intensity of his experience—whether emotional, affective, conational, or otherwise. This clear statement has been beclouded by sundry differences of opinion as to the exact quality of experience usually involved; but, so far as the actual evidence goes, there has apparently never been an instance in the literature where the central issue has been in doubt. In spite of this evidential consistency, a number of authors for theoretical reasons have ignored or deprecated the subjective side of the equation; and there are textbooks currently in use which treat the relationship as slight, or uncertain, or virtually non-existent.

Though probably most general textbooks do not yield completely to anti-mentalistic interpretations of the PGR, a brief review of the experimental facts, to which I can add some data of my own, may not be untimely.

THE EXPERIMENTAL EVIDENCE

The early assertions of Peterson (10), Veraguth (14), and others, were based on experimental studies; but not until somewhat later do publica-

tions give data in a form which can be statistically evaluated. Thus, Peterson's statement in 1907 that "every stimulus accompanied by an emotion produced a deviation of the galvanometer to a degree in direct proportion to the liveliness and actuality of the emotion aroused," is not bolstered by published figures, and, in the light of later evidence, appears to be an exaggeration.

In 1910 Starch (12), perhaps the first to do so, published tables adequately buttressing his view that "the degree of intensity of emotional experiences corresponds very closely with the amount of deflection." His subjects were asked to grade the effect of the stimuli to which they were exposed as: none, weak, medium, strong. In one experiment eight subjects judged in this manner the sound of a bell, all their judgments falling into the last three of the four categories. Corresponding to their judgments of weak, medium, and strong, the average galvanometric deflections were respectively, (in centimeters on his scale), .25, .52, and .62. In another experiment, where actual stimulation was combined with expectation and his six subjects were thus required to make a more difficult judgment, the average deflections for weak, medium, and strong reactions were .25, .92, and .72, and so not quite linear. In a third experiment, where comparisons were made between three different kinds of stimuli—an odor, a pinching pressure, and a pin-prick—the average deflections in the same order were .19, .45, and .63. Combining all his results, he found that the average deflections for weak, medium, and strong experi-

ences were .23, .63, and .66. Starch did not carry his analysis beyond this point; but his tables of individual results allow a correlational assessment of ten cases in which judgments of weak stimulation can be set over against judgments of strong or medium in the same experimental session, and according to my calculation the correlation between judgment and galvanometric deflection (corrected C) is .81.

In 1911 Wells and Forbes (16) published detailed tables showing the average amount of galvanometric deflection accompanying four categories of intensity of experience as judged by two subjects, who were evidently practised introspectionists. Words were used as stimuli, in lists of from 12 to 72. According to my analysis of the fifteen experiments in which the data are adequate for such treatment, when the average deflections are ranked over against the four categories of subjective intensity, the correlation (corrected C based on a 4×4 table) is .93.

In 1924 Honoria Wells (17) reported figures on three subjects who estimated their degree of alertness at specified moments in an experiment involving choice. "These estimates were made in 316 cases: subject A yields 119 correct estimates out of 142 attempts, *i.e.*, 83.8 per cent; subject B, 72 out of 96, giving 75 per cent; subject C, 51 out of 78, *i.e.*, 65.4 per cent. In total, of 316 cases 242 corroborated the reports." The indicated correspondence here is, of course, between subjective estimates and galvanometric deflections. As I understand the report, the subjects estimated their degree of alertness as greater than, less than, or equal to the criterion experience. Assuming an equalized distribution of the three possible categories, the chance correspondence between judgments and deflections would have been 33.3 per cent; since the actual correspondence was,

for the total, 76.6 per cent, the correspondence in excess of chance was 43.3 per cent, on this assumption. The correlation coefficient to be inferred from this degree of correspondence is .82.

In 1925 Wechsler (15) published an extensive study in which, among many other things, he reported on the correspondence between galvanometric deflections and subjective judgments of the emotion-rousing value of a set of stimulus words. He asked his subjects, after they had been presented with a list of thirteen words, to judge their emotional value for them on a five-category scale. Two lists were used with a number of subjects. The average deflections ranked against the average judgments yielded, on Wechsler's calculation, correlations of .59 and .67.

In 1926 Syz (13) reported an experiment in which the procedure was to read out a list of 50 words to a subject attending without making an oral response. At the conclusion of the reading, during which galvanometer records were taken, the subject was asked to indicate which words had aroused a conscious emotional response. This procedure was applied to 50 subjects. Syz was impressed by the infrequency with which subjects mentioned words which had produced a galvanometric deflection, and argued from this fact that the subject's report was unreliable. Neither the method of recording the galvanometric data (frequencies rather than magnitudes) nor the method of eliciting the subjects' judgments was designed for greatest sensitivity. It is therefore highly interesting, especially in view of the conclusions, that a significant positive correlation emerges from the data published by Syz. A table of his gives in percentages the frequency with which each word in the list produced a galvanometric deflection and a report of subjective emotion. If these figures represent average response-

intensity, then for the 50 words the correlation between galvanometrically indicated intensity and subjectivity is .45.

In 1927 Bartlett (1) published a table showing the relation between the degree of hedonic tone experienced by one subject and the degree of his galvanometric deflections. The stimuli were 24 pictures in an art book. This 4×4 table yields a correlation (corrected C) of .78.

In 1929 Cattell (2) published a table in which five grades of intensity of experience, as judged from subjects' reports, are compared with magnitude of corresponding galvanometric deflection. These figures are averages based on a large number of subjects, and the experiences are divided into those of conation, feeling, and cognition. In all three classes of experience, the correlation between the two measures is 1.00.

In 1930 Patterson (10), who employed a variety of stimulating conditions meant to cause an emotion of surprise in her subjects, reported the following correlations between subjective estimates of intensity of the experience and the galvanometric deflections for eight individuals: .53, .58, .59, .66, .72, .80, .86, .88.

In 1931 Dysinger (4) reported an experiment in which 3 word lists of 50 words each were presented as stimuli to 13 observers. Their intensity judgments were given immediately after exposure to each word in terms of a five-point scale ranging from very pleasant to very unpleasant. Though hedonic quality was not distinguished by the galvanometer, the subjective estimate of intensity corresponded with the magnitude of the deflection in 25 of 37 cases. A closer analysis of the detailed tables reveals that the relationship is really quite strong. When the galvanometric measures and subjective judgments in just adjoining intensity cate-

gories are compared (104 cases of correspondence between higher PGR and higher rating, as opposed to 13 cases of higher PGR coinciding with lower rating), the correlation (corrected C based on a 2×2 table) is .87.

Perhaps there are published studies bearing upon the point at issue which yield a negative correlation between the two dimensions under consideration; but, if so, it has been my misfortune to overlook them.

In harmony with the foregoing are the results from an experiment of my own, which need not be described in any great detail, since it presents no particular novelties. In 1948 I took galvanometric records on a large number of women students enrolled in a psychology course at Meredith College. In individual sessions they were presented with a series of adjectives printed singly on small cards which were held before their eyes one at a time. They were instructed to regard these adjectives as completing an implied question, namely, "Are you (whatever the adjective indicated)?" and to respond to the question with a silent yes or no. All the adjectives described personal qualities, as is evident from the list, which is here given in the order of presentation: *cheerful, adventurous, graceful, attractive, sympathetic, friendly, kind, intelligent, lovable, polite, glamorous, innocent, obedient, virtuous, generous, beautiful, calm, sweet, honest, brave*. The first word in the list was serviceable merely as a buffer, and was left out of account in the correlations. Galvanometric records were obtained from 53 subjects. One subject was excluded from the final list because of an extremely large reaction to the word *honest*. She volunteered information some time later which made it clear that the word had touched the heart of a very serious problem which doubtless distinguished

her sharply from the rest of the group. On the basis of 52 subjects, then, divided according to their laboratory hours into two sub-groups of 27 and 25, a reliability coefficient was obtained for the PGR of .77. After all had served as subjects, they were asked in a class session to try to rank the emotional value of the list of words (exclusive of *cheerful*) for an average student placed in that situation. Rank orders for the words were furnished on this judgmental basis by 51 of the subjects at a period from 1 to 12 days after their experience. The reliability coefficient for these rankings, when the subjects were divided as above, was .86. The rank order correlation between the judgments thus obtained and the galvanometric reactions was .76. Corrected for attenuation, this figure reaches a value of .94.

The accompanying table sums up the results of all these studies in terms of correlation coefficients, as obtained either by the experimenters themselves or by myself on the basis of the available data.

TABLE
CORRELATIONS BETWEEN PGR AND EXPERIENCED INTENSITY OF STIMULATION

Starch (1910)	.81
Wells and Forbes (1911)	.93
Wells (1924)	.82
Wechsler (1925)	.59, .67
Syz (1926)	.45
Bartlett (1927)	.78
Cattell (1929)	1.00
Patterson (1930)	.53, .58, .59, .66, .72, .80, .86, .88
Dysinger (1931)	.87
McCurdy (1948)	.76 (corrected, .94)

DISCUSSION

Rare as uniformly positive results are in experimental psychology, one might expect such a relationship as the one brought out by these investigations to be highly valued. I doubt that it

is. Since the study by Dysinger in 1931 I do not know of a single study which has treated it as a central topic. This may be accounted for in large measure by the slashing onslaught of Landis on every trace of mentalism in galvanometric work. At any rate, while the instrumental questions and bodily reactions associated with the PGR have been continuously investigated since that time, little attention has been paid to the very sturdy fact of the connection between the subjective evaluation of stimuli and the electrical events detected at the surface of the skin.

One important study in this period might be regarded as an exception, namely, the 1940 paper by Hovland and Riesen (6) on galvanic and vasomotor response as a function of the stimulus. But apparently their intent is to show that the physical energy of the stimulus can be considered apart from the subjective reaction as determining the extent of the galvanometric deflection. In their words, "The level of reaction, long known to vary with concomitant degrees of affective tone, can also vary with intensities of stimulation which produce at most only minimal changes in emotional experience." While they do not disregard the subjective component entirely, and do freely admit the drift of the more psychophysical studies, their conclusions seem to deny any fundamental importance to the conscious reaction.

But is it not fundamentally the assessment of the stimulus by the subject which determines the magnitude of the galvanic reaction? It is pointed out here and there in the studies cited above that when subjects failed to notice stimuli there was no galvanic reaction; and of course where they do notice the stimuli there is the close relationship described between the size of the reaction and the placement of stimulus-

effect on a continuum of subjective intensity—a placement which the subject is able to report verbally, but which I should certainly argue is not dependent on the verbalization.

In Hovland and Riesen's study there is a curious circularity. The sound and shock stimuli are selected because they are subjectively easy to distinguish. Their intensities are then stated in terms of microamperes and decibels. Finally, a linear relationship is demonstrated between the stimulus intensities as thus measured and the magnitude of the galvanic reaction. To all appearances, then, the galvanic reactions are a function of the physical energies applied, without reference to any mediating state of consciousness. But we arrive at such a conclusion, if we do, by dropping out of consideration the original criterion of selection, namely that the stimuli are subjectively distinguishable, and by overlooking some observations on the reactions of subjects during the experiment. In regard to the shock stimulations the authors write: "The subjects differed only slightly in their reports of the nature of sensations represented by these stimuli. The strongest was generally termed unpleasant and mildly painful, and the weakest was clearly discernible, but not at all painful. In all cases discriminations of greater or lesser intensities between adjacent settings were easily made" (6, p. 108).

Where the stimuli are simple energies, an appearance of isolating their physical properties and relating these to the galvanic response can easily be achieved. One makes use of some standard piece of measuring equipment, and then, after getting the subject to regard the stimuli simply as intensities (not indicators of meanings), proceeds to show that the galvanic reactions run parallel with the instrumentally determined strength of stimu-

lus. But when the stimuli are words, it is much less easy to escape the implication of the subject's conscious states. There is no handy measuring instrument for determining the intensity of these stimuli. One could be constructed, however, by building into a machine the results of mass judgments on the emotional value of words; and some experimenter might then be able to come to the conclusion that his "objectively" measured stimuli were producing their effects quite apart from any conscious processes in the subjects.

The suggested logometer might be used with fair success over a wide area after one calibration. Consider the reliability coefficient of .86 for my Meredith women in 1948. I obtained exactly the same reliability coefficient for a University of North Carolina population of 52 student judges, half of them men, half women, in 1950. To be sure, their correlation with the Meredith group was lower, but still significant, namely .70. Reference should also be made here to the emotional intensity values found galvanometrically by Whately Smith (11) for a word list applied to subjects in England, and the confirmation of these values for a number of the words by Jones and Wechsler (7) when they were applied several years later to subjects in this country. Do we actually know that there is less stability in the subjective assessment of the intensities of words, which we do not measure instrumentally, than there is for the so-called physical stimuli, which we do? It is a question worth studying.

The temptation is very great for psychologists, striving to be natural scientists, to leave out of consideration the essentially psychological features of their universe of discourse. Already in 1909 Veraguth found it necessary to insist on the psychic component in the psychogalvanic phenomenon. "Auch

bei den sensorischen Reizen ist eine psychische Komponente als notwendig zur Hervorbringung des galvanischen Reflexphänomens anzunehmen. . . . Die galvanische Reaktion auf akustischen Reiz ist also eine Elektive; sie zeichnet diejenigen akustischen Reize aus, welche die Aufmerksamkeit der V.-P. erregen" (14, p. 35). When investigators turn aside to discover the relationships between the galvanic reaction and other variables than the psychic (which, of course, is a perfectly legitimate and interesting enterprise), they come up with such correlations as .28 with EEG (3) or from .32 to .71 with vasoconstriction (6) and seem to be well satisfied with such evidence of synergy within the nervous system. Is there any respectable reason for being any less well satisfied with the consistently higher correlations between this much-investigated reaction and the subjective events which attend it? A refocusing of attention on this connection, after so many years devoted to refinements of the electrical measuring devices, might lead to some important discoveries along the lines marked out by Peterson, Wechsler, Whately Smith, and especially Veraguth.

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KNOWLEDGE IN MODERN PSYCHOLOGY

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Schools of psychology are discriminated by the place that they give to knowledge. The older schools made knowledge central. Some later schools rule it out altogether, subordinate it to responses, or postpone its discussion until conditioned reflexes are exhausted. They either take knowledge for granted or deny it. The differences develop from the hypotheses that the schools accept. Agreement or at least intelligent controversy will be furthered by stating the axioms, and testing them by known facts.

An axiom that seems implied by one school, the behaviorists, is that all appreciation is limited to the man who experiments or observes, and that the man who acts or is experimented upon knows neither that he is acting nor the occasion that evokes his response. The behaviorists do not deny the existence of knowledge for such a denial would eliminate the observer's awareness as well as the actor's, and it is upon the observer's experience that they build their doctrine. They do not state this difference, but it is implicit in their conclusion. The assertion developed from Watson's desire to justify the possibility of animal psychology against Titchener's insistence that introspection was the only method in psychology and that as animals could not introspect they could have no psychology. Watson turned the tables by denying the existence of consciousness on the military principle that offense is the best defense. Animal psychology long ago justified itself and does not enter the present picture.

Among the behaviorists Skinner has given a different approach by dividing

knowledge into private and public. He cites a toothache as an instance of private experience of simple sensory origin. None other than the man subject to it knows of it except through the description of it or from the contortions or grimaces. A true behaviorist does not admit the existence of memories, perceptions and similar elements of knowledge that are not shared, but now and again he lets slip a reference that implies them. Private is none the less a convenient term even for the behaviorist who does not believe that private events exist. Skinner would reject the possibility that private-public is the equivalent of subjective-objective or idea-matter. That goes with the denial of the conscious. He suggests that the distinction is related to the special problem of representing mental processes in words. A third relationship is between generally accepted truth and a divergent individual opinion. Important, too, is the difference in reference, although this might be important only if one grants an awareness. Ideas regarded as personal are private; those attributed to the physical world are public. The distinction of public and private as a means of avoiding disagreement as to the existence of consciousness has its limitations, for it is difficult to think of private factors, if consciousness is eliminated.

Behaviorism has been connected in recent writing with operationism, as in the symposium in this JOURNAL (4). Operationism was suggested by Bridgman, the physicist, as a means of avoiding the paradoxes that appeared in the Michaelson-Morley measurements of the displacement of light in relation to the

moving earth. It would appear that measurements of velocity with and against the motion of the earth should differ, but no difference was detected. One suggested explanation was the doctrine of relativity. Bridgman argued that it was an instance of the fact that a measurement varies with the conditions under which it is made. Everyone had assumed that the movements of the earth would affect measurements of light from the sun as measurements on a moving train would affect light from a street lamp. The fact that the displacement did not appear in the measurement proved that movement made no essential difference in the observation. Bridgman generalized the case to argue for great accuracy in assuring identity of conditions for all instances that were to be combined in a single statement. Any statement of a result should contain a description of the conditions under which the observation or measurement was made, for it would be valid only with essentially the same apparatus and similar factors. Included in the control must be the subjective attitudes, for the latter may mislead as much as the physical factors.

The precautions of operationalism should be applied rigorously in psychological reports, possibly more closely than in physics, for the terms in psychology are less rigidly defined. The word "learning" is applied to numerous widely divergent operations of animal and man. It has been measured by success in repeating series and separate units and also by recognizing elements or wholes, by using meaning as a measure, or the words. Generally the method and the material are indicated in the statement, but some generalizations omit them to the confusion of the worker and his reader.

Wider generalizations may be tested by tenets of operationalism. Such a

universal negative as that there is no consciousness would be rejected at once when operations are attempted. No induction is ever complete. You find a difference of opinion between two authorities as to the relative importance of public and private knowledge in the development of science. Skinner insists that all science is public, apparently both in origin and result. Bridgman, on the other hand, says: "I believe . . . that a simple inspection of what one does in any scientific enterprise will show that the most *important* part of science is private" (4, p. 281). No one can question his right to an opinion on the point. Possibly Skinner means public in reference, while Bridgman means private in origin.

The sharp difference in fundamentals between the behaviorists and the other schools suggests a study of the presuppositions of each, as stated or implied. It seems to be a first principle of the behaviorist that an observer has an advantage in accuracy over the man watched. In fact the behaviorist imputes to the actor or man observed no capacity at all for an accurate report or even for seeing. When the experimenter records a result he does not ask the man who responds anything of the preliminaries of the response. The relative accuracy of observer and actor has never been investigated, and incidental report and popular discussion do not raise the question. Certainly there is nothing in current belief that favors the observer. A man is more likely to remember where he left his umbrella than will be his companion at the time it was mislaid. He is more interested and revives kinaesthetic in addition to the objective senses in recall. Disbelieve the actor and confession would have no weight in court. Were it necessary for an individual to find a companion to report on his acts before he could know of his past, all men would

be reduced to inefficiency. This advantage of the observer over the actor is implied rather than asserted, but would be the premise behind many of the behaviorists' statements.

The second positive assertion of the behaviorist, from which the superiority of the observer follows, is that the individual has no awareness of any kind at any time. Consciousness does not exist. Both animal and man can be treated as if they were merely means of transferring stimulations from the sense organs to the muscles for the purpose of arousing movements. Some of the earlier men suggested that knowledge might arise during some stage of the response, but the most recent men neglect knowledge entirely. Hull hopes that when we know more about reflexes we may discover something of the more involved connections in thinking. Following Watson, he discards consciousness as a myth transmitted from the pre-behaviorist dark ages. It all suggests the baby-sitter's definition of an infant as "a machine for changing good milk into bad water." Both definitions are undoubtedly true, but possibly one-sided.

Decision on the truth of an axiom is difficult. An axiom for geometry is a proposition that everyone accepts. For the Platonist it was a revelation common to the divine and human mind in its very essence. Very few axioms in practice stand the test of universal acceptance. Application of operationalism offers little help. It is difficult to arrange specific responses that are appropriate and it might be still more difficult to be sure that the conditions are the same for different tests. Ask a psychologist if he is conscious at the moment and his answer will depend upon his school. This is one of the conditions that determine his reply. We can attempt to break up the general question into simpler parts.

One can ask: Can you tell the difference between being conscious and unconscious? More specifically can you tell that you have been asleep? To this most would answer yes. A purist might argue that this is comparing a present condition with a past. In the same spirit one might say that all comparisons are of a present with a past experience. One cannot compare simultaneous tones. They fuse. One cannot be fully aware of two different colors or lengths at the same time. One must always pass from one to another to compare. Some might argue that one knew sleep from waking not on the basis of consciousness, but by recalling the quiescence or slowed breathing periods as marking sleep or in noticing motor changes that accompanied waking. These too would be forms of awareness and could not be evidence of a negative answer to our question.

The problem of consciousness has been discussed usually with reference to concrete experiences. Does the individual have particular conscious processes when he sees, recalls and reasons? Do sensory stimulations give rise to conscious processes and are they recalled? Controlled observations on these questions are numerous and go back to Aristotle. Striking instances are the reports of Galton and his followers from the 1880's. They were especially careful, for they found that men differed in the senses they used in recall, and related them to different capacities. Even more definitely under control was Külpe's study of the differences between images and sensations. He worked with several men who later became distinguished psychologists. All agreed that they had specific images and could distinguish them from sensations. These are but two of many investigations that have reported imagery or consciousness. No actual in-

investigators have denied that they exist. Denials have all been dogmatic general assertions.

The studies mentioned have been predicated upon the assumption that private experience could be reduced to discrete units. A more recent school, whose rise was contemporaneous with the behavioristic, holds to the existence of private experience, although it denies atomistic components. This is the Gestalt movement, initiated by Wertheimer and now most prominently represented by Köhler. Wertheimer was led to it by experiments on visual movement. He concluded from observation and experiment that movement was an immediate experience, the same when aroused by a stimulus moving across the retina, and by the successive stimulations of neighboring points on the retina as in moving pictures. Following Mach and von Ehrenfels the school asserts that all perceptions and intellectual processes are similar immediate experiences which they call *Gestalten*. A square, a circle, a melody are wholes that determine their parts rather than are determined by them. The school denies the existence of separate sensations, and also denies the importance of associations or conditioning which are the stock in trade of behaviorists. The Gestalt school makes awareness or consciousness a datum of psychology and of knowledge.

It seems that aside from behaviorists, psychologists accept awareness as basic to knowledge, and knowledge as important. A behaviorist is little inclined to accept particular events and would probably find knowing the difference between being awake or asleep as irrelevant. He accepts only reports of the observer, not of the man observed, and will permit no questions of how knowledge arises or of its adequacy, aside from a statistical study of errors of observation.

The problem is complicated by the fact that knowledge in reference or content may take six forms, partly distinct, partly overlapping. In most philosophy the three fundamentals are the thing or external existence, the nervous system, which must respond if the thing is to be known, and the awareness that accompanies the stimulation. Three others are subordinate, but have been made fundamental by certain schools. These are the word, meaning or reference, and movement. The thing is the fundamental end of knowledge. A few realists assume that it can be known directly. This probably dodges the problem rather than solves it. The object is seen to be outside the body and is known only through stimuli to sense organs. How stimuli affect the sense organ and induce awareness in the individual is a psycho-physiological problem and must be faced even if attempts to solve it in experiment or theory have made little progress. The thing and man's idea of it are in mental content the same. No qualities or forms exist except for the thinker. The thing is the idea referred outward, the idea is the thing in the process of knowing. The word is a symbol on the thought side that represents the thing. Meaning is the fact of reference and applies ideas to things and to other ideas. Movements accompany or follow most stimulations and many mental processes. They arise through the thing-idea, and add quality to many ideas. Expressive movements reveal the thought and especially the feelings and emotions of the individual to the observer.

In matter and representation each of these six phases is approximately the same. Each is effective in a slightly different way in representing the common referent, although in content, where that can be compared, they are vastly different. Different schools have

denied the existence or importance of all but words, movements, and the nervous system. The same schools have made certain aspects exclusive. Berkeley long ago denied the existence of physical things. The behaviorists deny awareness to the actor although they imply its existence for the observer. Morris and the Gestaltists deny meaning, although the Gestalt probably includes both meaning and content. Morris also identifies reality with words, and it is difficult to see that a word would have value without meaning. He slips it in as semantics. Carnap and the Philosophy of Science men accept only things and words as real. Words do duty for all forms of awareness—how, they do not say. The nervous system and movements alone escape repudiation by all the groups, and these are often given minor roles. Some psychologists leave the nervous system discussion to the biological sciences. Some give movements a dominant place in explaining mental life, others subordinate them to control by consciousness. All consider them.

If all six must exist and overlap in function, the question might be raised why individual thinkers make one or more exclusive, and then reject others. One might suggest that some are rejected because they offer difficulties to experiment or to the formulation of the results of experiments. Movements can be recorded and measured accurately, while sensory processes require other methods. Weiss said that a sensation came but once so could not be subjected to statistics, hence could not be part of science and so did not exist. But a physical event is also unique and so suffers from the same disability. Private events are grouped by approximation and so are not different from physical facts as presented to operationalism or to Heisenberg's theory. One cannot deny the existence of

a thing because it does not fit into a theory, or into the usual methods of measurement. It is much better to develop theories to fit facts after finding methods that will measure them.

An attempt to find common acceptance of the divergent opinions is obviously demanded, at least as a working basis of common effort. Where we have six factors that refer to the same thing or function, it should be possible to arrange them in order of primacy. Three are diverse aspects of the same objective event: the external stimulus, the reaction of the nervous system and the idea. This may be regarded as a cause and effect or successive response. Without the first two the third would not exist. Only the third stage is known, if we accept the conventional theory of all but the behaviorists. Object and nervous system are discovered only by exhaustive experiment and construction in thought. To immediate knowledge, idea and object are one. Naïvely it seems that the one is the object, but interpretation shows it is also idea. The real thing is a construct developed by the sciences. The idea represents it and we are aware of that alone. The nervous system reveals itself neither in content nor in reference. We are not aware of it as part of knowing. It is purely an objective construction. Although words may represent anything, they are valueless without reference to the real. Similarly meaning and movements as phases of knowing are but shadows of the object-idea.

The complication introduced by the behaviorist seems to arise from the fact that when two people are together, perceiving is twofold and he accepts only the awareness of the observer. If A is experimenting with B or observing him, the behaviorist assumes that B is like an animal and can report nothing because he can perceive nothing. A on the other hand is aware of the object

and of B in relation to it. B can speak for A to understand but cannot understand himself. Of course A can see only that an object touches B's skin and note the movements of exploration; he cannot be aware of the qualities aroused in B. The common sense assumption is that A and B have the same impressions from the same object although no comparison is possible. The behaviorist assumes that A is aware of B and of his response; otherwise there would be no knowledge. Behaviorism is made up of these reports and unless they consist of nothing but words, acts must have been observed. Since A and B can change from observer to observed, awareness must be entirely denied or granted to both. The problem of knowledge becomes important in the latter case.

Study of the individual from the outside has been well advanced by Pavlov, Liddell, Hull and his students, Tolman and many others. The facts accumulated have great value and are sure to find a place, whatever systematic formulation finally prevails. The inside or the private life of both observer and observed must also find a place in psychology in order to make sure that the awareness of the observer corresponds to the event, and in order to find out whether the awareness of the observed involves antecedents other than the immediately preceding physical ones that may modify the response.

Apart from the bearing on responses, knowledge is an important part of psychology. Methods of study are more difficult to apply than the ones used for responses. Nevertheless much progress had been made in discovering laws before the behaviorist assured us that consciousness did not exist, and con-

tinues to be made in spite of his scepticism. Many measurements can be translated into physical reactions and are as definite as a reaction time, more constant than most responses that are measured. Less accurate measurements of so important a process as reasoning are better than perfect measurements of reflexes. This plea is for giving knowledge a place in psychology as large as that assigned to movements. By this is meant knowledge as a private experience. If one may borrow from the assurance of the behaviorist as to what is or is not in another man's head, we may assert that mere reflexes or words subarticulated in a man's throat, especially since he is not aware of them, would not justify the emotion a behaviorist evinces in his denial of consciousness.

This argument is not so much one on fundamentals as a plea to the psychologist to consider knowledge and not merely responses, and to study man from within and not restrict methods to those that can be used only with animals. One might even go along with the philosophers to ask what is the content of things as perceived and how we become aware of them.

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RESEARCH METHODS IN CULTURAL ANTHROPOLOGY IN RELATION TO SCIENTIFIC CRITERIA¹

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I. INTRODUCTION

Universities and colleges throughout the country are becoming aware of a need for integrated courses of study in the social sciences. In the process of coordinating the various fields, differences in approach and points of view have inevitably arisen. The experimental psychologist, in particular, finds it difficult to accept the apparently unscientific research methods of the cultural anthropologist.

Some of this difficulty results from failure on the part of the psychologist to consider the present stage of development of anthropological method in historical perspective. Much of the difficulty lies in the anthropologist's practice of presenting his work in the form of brief journal reports of a narrative, descriptive nature. Frequently these reports fail to include such essentials as a statement of the problem under investigation, an explanation of the plan of the research and a presentation of the data from which the conclusions are drawn. The average psychologist, whose acquaintance with cul-

tural anthropology is necessarily limited to casual reading of the journals, is quick to conclude that the research methods of anthropology are highly "unscientific." Without reading further he tends to reject *in toto* the findings and conclusions of cultural anthropology.

It is especially unfortunate that this situation should exist since the psychologist and anthropologist alike are faced with the problem of studying complex and variable human phenomena. Both must take their human material as it comes, must devise special techniques for isolating, varying and controlling human factors and must struggle to make quantitative measurements of these factors. It is the purpose of this paper, therefore, to attempt to increase the psychologists' information regarding the research methods of anthropology by presenting first a brief discussion of the development of anthropological method and an evaluation of present method as it appears from a study of reports appearing in a current journal of anthropology. With this as background, we shall in Part II examine more closely some of the specific techniques and practices of cultural anthropology and relate them to the scientific criteria which the experimental psychologist expects them to satisfy.

A. *The development of method in cultural anthropology.* All of the sciences of man—biology, psychology and anthropology—began their studies with the collection of random observations which were presented in the form of descriptive anecdotes. The biologist

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looked planlessly into his microscope and described what he saw there in common, everyday terms. The psychologist similarly looked unsystematically at people around him and described his observations. The anthropologist, too, passed through a stage in which he visited a "primitive" society and observed through the eyes of his own culture the "oddities" and "odious practices" found in the primitive group. The anecdotal descriptive method served in its time to suggest problems and to indicate possible relationships for systematic study but, lacking hypotheses and objective standards for the collection and presentation of observations, it could not lead to generalizations and predictions in the manner of systematic scientific experimentation.

Biology and experimental psychology have now reached the scientific stage. Anthropology is still partially in the anecdotal descriptive stage. One of the reasons for this is, of course, that the task of the anthropologist is in some ways more difficult than that of either of the other sciences. Ehrich has recently described this task as follows: "... the anthropologist must not only strive to obtain a cross-sectional view of modern man and his ways of life, but he must also find out what has happened to man in the past and what he has produced" (2, p. 343.).

The anthropologist, then, takes on the tasks of numerous specialists in our society and carries them out in the difficult laboratories of widely different cultural groups.

Though cultural anthropologists are not agreed on the extent to which anecdotal, descriptive methods must still be used, some feel that certain areas cannot yet be attacked by narrow scientific methods. Such problems as the integration of various aspects of culture, the functions and motivations of

cultural forms of behavior, the historical development of various aspects of culture, and cultural processes such as socialization and acculturation, are said to be still in an exploratory stage and not yet ripe for precise study until more data are collected. Though these data are to be collected as objectively and systematically as possible, the crucial variables and conditions must be determined before they can be isolated, varied and measured. For example, Mead states that the anthropologist must first collect data on the total socialization process and then he may make precise determinations on focal points within that process (14, p. 674).

Furthermore, when the relevant variables and conditions are not sufficiently known, it is difficult to determine how observations can be reported in standardized form. They cannot be grouped into meaningful categories that are based on similarities and that disregard differences in conditions, since the crucial similarities and differences which should define the categories are not easy to determine. Consequently Herskovits comments that, "any report he [the anthropologist] can phrase must of necessity be anecdotal and should run with presentation of his data since each item of his materials is gathered under circumstances that differ from those in which every other item was obtained" (7, p. 81).

Granted, then, that anecdotal forms of report and fact-collecting without precise determination may still be necessary in some areas, there is no doubt that in other areas the anthropologist has gathered sufficient material on important conditions and variables to be ready for more careful study of specific problems with a view toward scientific generalization and prediction.

B. Reports of field research appearing in a current anthropological journal.

To the experimental psychologist, accustomed to a clear statement of problem, of the plan of the research, of the specific experimental techniques used, and to a relatively full statistical presentation of the data, the reports of field work in the anthropological journals appear planless, sketchy and highly subjective. In order to determine the extent to which this impression is justified, the writer examined and evaluated all the articles based on field research in cultural anthropology which appeared in a current journal over a period of 15 months. When theoretical articles and studies in physi-

cal anthropology were excluded, seven studies remained which were based directly on field research. An analysis of these with regard to (1) the statement of the hypothesis and of the specific problem designed to test it and the general plan of the study, *i.e.*, what factors were isolated, varied and controlled and by what means; (2) the techniques of collecting data, the form and comprehensiveness of the report of the data and the conditions under which the data were obtained; and (3) the validity of the conclusions and generalizations made on the basis of the data; revealed the following picture.

1. Statement of hypothesis, specific problem and plan of study

No. of Studies

Clear statement of hypothesis and problem	1
Implied hypothesis, vague general statement of problem	4
No statement of hypothesis or problem	2
Precise description of plan	1
Description of factors isolated and varied and of a few of the factors controlled	3
No description of plan	3

2. Techniques of collecting data, form and completeness of report, conditions under which data obtained

No. of Studies

Collection of data by direct measurement	1
Collection of data by test procedure	1
Collection of data not described	5
Complete data statistically presented	1
Partial data, presented in narrative form	4
Data interpreted only	2
Size of sample in relation to total population reported	2
Size of sample not reported	5
Composition of sample in relation to total population reported	1
Composition of sample described but not related to total population	3
Composition of sample not reported	3

3. Validity of conclusions and generalizations drawn from data

No. of Studies

Valid conclusions based on data reported	1
Probably valid conclusions though full data not reported, broad generalizations made beyond data	2
Validity of conclusions questionable since variables inadequately described and full data not reported	4

It is apparent from this summary that, though there is some "sense of problem" in most of the studies, the problems and hypotheses to which they are related are not clearly described, leaving room for error in the reader's interpretation of the purpose of the research. Lack of precise description of the variables involved, of standard ways of measuring these variables, and of full presentation of the data, leave the reader in some doubt both as to the validity of the conclusions and the objectivity of the data reported. Finally, the frequent failure to report size and composition of sample in relation to the total population throws further doubt on the reliability and validity of the generalizations made. Lack of space may account for some of these shortcomings, but a brief description of the important variables dealt with and the size and composition of the sample observed should be possible even where space is at a premium. Furthermore, it is precisely in short studies, rather than in full-length ethnographic reports, that one would expect to find clearly delimited problems related to specific hypotheses.

In order to determine whether the failures and omissions noted really reflect inadequate research techniques or whether they are simply a function of poor scientific reporting, it is necessary to make a more extensive examination of the anthropological literature. For the purpose of such an examination, we shall report some of the current methods and practices as the anthropologist describes them and attempt to relate these techniques to the scientific criteria of objectivity, pertinency and dependability.

II. METHODS OF ANTHROPOLOGY RELATED TO SCIENTIFIC CRITERIA²

A. Techniques related to objectivity. Precise standard measurements are used in anthropological research wherever overt aspects of culture lending themselves to quantitative description are observed; for example, objects of material culture, technical activities, aesthetic products, etc. Measurable facts may be merely supplementary to the description of the whole cultural group, but they are often used for correlation with less tangible aspects of culture or for cross-cultural comparison. Where measurement is made, the usual statistical methods of reporting the data are used. Where quantitative description is difficult the data are sometimes presented in terms of charts, a device suggested and used by Malinowski (12, pp. 214-218; 13, pp. 382-482). Such cultural phenomena as economic transactions, gifts and presents customary in a society, social and ceremonial systems of magic, connected series of ceremonies, types of legal acts, etc. are so presented. Malinowski also urges the standard use of a genealogical census, of maps, plans and diagrams to illustrate ownership of garden land, hunting and fishing privileges, etc. presented together with statistical documentation.

Measurement in terms of the fre-

²No detailed report of the methods and field techniques of cultural anthropology has appeared recently in the literature. Such a report is beyond the scope of this paper. We shall attempt merely to relate to scientific criteria such methods and techniques as are described in the literature or demonstrated in the field research cited. Though incomplete, our discussion may serve to bring together the various suggestions of anthropologists regarding the application of scientific procedures to cultural data.

quency with which, for example, a certain material manifestation or a given trait or type of activity occurs may be found in the literature for numerous overt aspects of culture.

Tests, questionnaires, rating scales or set interviews, where situation and response are standardized, appear to be infrequently used in anthropological research and their use at present is limited to psychological studies. Tests and projective techniques in which responses are relatively free are more frequently administered.³ Results of tests requiring set responses are statistically treated in the usual way while results of those allowing free responses are grouped into categories based on uniform criteria and analyzed in terms of frequency of each response category.

Uncontrolled observations and free interviews are apparently the most frequently used techniques in anthropological research. The sequence of events and overt behavior are usually studied by observation with simultaneous note-taking, verbatim records or camera recordings, or by participant observation in which notes are taken afterwards. Functions of events or ceremonies or the motivations of behavior are studied by free interviews with informants, with verbatim record if possible, in which the informant may answer specific questions and may volunteer information and opinions regarding a set topic or may provide casual information on any phase of the culture or on his reactions to certain situations.

³ For description of psychological tests of perception, reaction time, color discrimination etc., see (4). For use of Piaget type tests see (19). For studies using various psychological tests and projective techniques, see (20, 11, 10, 8). For use of the Rorschach technique, see (5), and for a survey of studies using the Rorschach technique see (1). For a slightly different type of test in which stories and pictures were presented and later reproduced, see (17).

The methods of uncontrolled observation and free interview do not themselves provide the precise measurement required for scientific standards of objectivity. However, if adequate numbers or groups are studied in enough situations so that a large sample of the same type of situation is obtained, and if many reports or observations of reactions to and meanings of that situation are also obtained, the anthropologist, like the psychologist, may compensate for difficulty in measurement by the use of many observations.

In addition, the anthropologist has devised certain checks to eliminate bias on the part of the observer or at least to indicate the direction of his bias. To assure completeness of observations so that the kinds of data observed are not affected by the ethnographer's bias, the Notes and Queries method may be applied to see that nothing is omitted. To standardize the aspects and specific phenomena of culture which must be covered, Malinowski stresses the importance of reporting the absence as well as the presence of customs and behavior, and of reporting rules about culture traits and patterns, as reported by informants, as well as the norms of behavior as observed by the investigator. More recently, Ford (3, pp. 154-156) has indicated a standard way in which rules and norms may be analyzed to avoid omission of important data.

For rules: analysis may be made in terms of the persons to whom the rule applies; the conditions under which it applies; the emphasis placed on the rules scaled from "must not" to "must do"; the behavior with which the rule is concerned; the positive and negative sanctions; and the meaning of the rules. For norms: analysis may be made in terms of conditions; persons involved; the behavior; the resultant behavior; and the rationalization of expressed motivation for the behavior.

Further standardization is made by the Cross-Cultural Survey (16) with regard to events which should be observed for a coverage of the basic data in a culture and for detailed studies of particular aspects.

To assure objectivity and completeness of particular observations, Malinowski advocates that records of verbal behavior be taken in native dialect, although others feel that this is a doubtful procedure unless the ethnographer is unusually fluent in the dialect. The question of whether the use of a dialect, of pidgin English, or of an interpreter is most satisfactory has apparently not been experimentally determined for any particular cultural group, although in some cases a reasonably controlled study might be undertaken.

Finally, Whiting recommends that the category in which the investigator is placed by the native group from the beginning to the end of contact should be reported to indicate the completeness with which he has been able to study all aspects of the culture (21). Whiting also indicates in a footnote to each aspect of his investigation of the Kwoma from which persons he obtained his information, and thus allows the reader to evaluate any bias resulting from the status, sex or age of the informants.

The objectivity attained by the methods of observation and free report of informants is somewhat increased by the techniques outlined above. The techniques and schemes for standardizing the data on which observations and information are to be obtained allow for the possibility of presenting some of the data, at least, in terms of frequency with which a certain event or type of behavior is observed. Using Ford's suggestion for quantifying intensity, the anthropologist may extend the analysis of his data beyond mere frequency determination and report

quantitatively the emphasis placed upon different rules. Comparison of the intensity of different rules within a culture or cross-cultural comparisons of the same rule are thereby made possible.

Numerous other schemes have been suggested for standard classification of special phenomena so that findings in a particular area of investigation may be understood by others working with this problem. Kluckhohn and Mowrer (9), for example, suggest a classification of eight personality determinants, arranged in such a way as to produce 16 cells. Any naturalistic determinant of personality can then be assigned to one of these cells.

A final way of obtaining and reporting data is by the collection of life histories or case histories. If these are simply retrospective and cannot be checked, they are of value only in obtaining information on conscious motivation and on the expressed values and attitudes of a particular person. Opler suggests taking the life histories of a number of individuals of representative and deviant types, annotated by a psychiatrist as well as by an anthropologist and obtained in a number of different groups for the purpose of cross-cultural comparison (18). Such data might serve a valuable purpose similar to that of introspective data gathered by the experimental psychologist, but would not stand the test of objectivity unless large numbers of life histories from each culture were obtained in a uniform way and were carefully analyzed according to standard psychiatric and anthropological criteria.

For longitudinal studies of personality development and the socialization process, Mead suggests the use of simultaneous life histories which examine a segment of a person's life and are supplemented by observation of his behavior (14).

It is apparent from this brief survey that, although narrative, subjective reports may be necessary to convey a full picture of a culture, standard objective ways of collecting and reporting some types of data have been suggested and are used by a number of cultural anthropologists.

B. Techniques related to pertinency: isolation and control of variables.

Pertinency, in the strict sense, is difficult to achieve in anthropological research where not only the human individual, but the physical, social and cultural environment must be taken as they come. Furthermore, the historical factors important in shaping man's cultural environment give rise to a whole set of variables which must be considered. The anthropologist, then, cannot achieve pertinency by the mechanical isolation and variation of single variables with control of all other factors, but must, like the psychologist, approximate it by studying groups which differ, insofar as possible, with regard to only one set of factors. For this purpose it is necessary to find groups in which the important factors have been sufficiently defined to allow precise study of limited problems by the techniques of isolation, variation and control. Numerous studies using these techniques relatively precisely have appeared in the literature.

For example, Leighton and Kluckhohn (10) in their study of the Navaho have varied the factor of contact with white people by choosing three different settlements within the Navaho group which represent three different degrees of white contact. The groups were similar with regard to historical, racial, social and religious factors, and with regard to such specific factors for the problem under investigation as methods of child rearing. The effect of white contact on the personality configuration of children, as measured

by the Rorschach and other tests, was then observed.

In a similar study, Hallowell (5), observing the Saulteaux, achieved unusual control of historical and cultural factors by testing, in some cases, members of the same family who resided in different areas and were subject to different degrees of contact with whites.

Nadel (17) also varied cultural factors, though less precisely, by testing two groups with "almost antagonistic cultures," that is, two cultures differing greatly in aesthetic and linguistic aspects. The economic and political life, material environment and stage of civilization of the two groups were said to be the same. In this study the effects of cultural differences on the quality of recall were observed.

Longitudinal studies of socialization and child development have provided the best means of controlling historical, cultural and relatively permanent psychological factors such as biological make-up and past experiences. Similarly, historical studies use essentially a longitudinal method to achieve control of racial and cultural factors. For example, the same tribe may be studied at different stages with regard to a certain variable such as economic organization or some other cultural form. These data are then examined in relation to differences in other specific aspects of culture.

To illustrate the extent to which the anthropologist can demonstrate a pertinent relationship as a test of a specific hypothesis in a different type of problem from those mentioned above, we may report Hallowell's study of the size of Algonkian hunting grounds (6).

The purpose of Hallowell's study is to test Cooper's hypothesis that land tenure adapts readily and swiftly in accordance with changing ecological conditions. The particular aspect of land tenure examined

to test this hypothesis is that of size of hunting grounds.

Two Algonkian groups which differ widely with regard to size of hunting territories are chosen for study. The groups are similar with regard to linguistic and cultural background, faunal zone, animals hunted, technical equipment used, way in which they dispose of their furs and standard of living.

Possible causal factors for differences in size of hunting territories are then studied.

First: demographic facts are obtained which show that the ratio of active hunters to total other persons in the group is the same for the two Algonkian groups and must, therefore, be ruled out as a determinant of size of hunting territory.

Second: cultural determinants are examined and it is found that there are no economic factors motivating accumulation of large tracts of land in either group, nor are there inheritance rules laid down in either group which specify the size of hunting tracts. Consequently, neither of these cultural factors can account for the difference in size. As none of these factors is responsible for the difference, Hallowell assumes that the abundance of game must be the main determinant of size of territories.

Since Hallowell presents this study to demonstrate the need for going beyond cultural data in the search for causal factors, he does not present the precise ecological measurements required to complete the study. However, the setup of this study shows the way in which the criterion of pertinency may be satisfied for a limited problem despite the impossibility of manipulating variables in a ready-made laboratory situation.

As indicated earlier, the anthropologist has not been able in all areas to limit his problems and manipulate specific variables for precise study. He is still occupied with the description of culture with a view to observing as many phenomena as he can and relating them to possible determinants. Consequently he not only observes behavior but tries to get at its special

culturally rooted motivations; he witnesses ceremonies and attempts to discover their function and place in the total culture; he notes customs and sanctions, and inquires into the reasons for their particular forms. Eventually he expects to gain enough information to allow the application of more precise methods for determining and quantifying pertinent relationships. Broadly speaking, however, the anthropologist who studies such complex problems as acculturation is still manipulating factors by the selection of individuals or groups similar in certain general respects but differing in others.

Herskovits (7, pp. 613-615) illustrates this procedure in his study of the acculturation process among the African and New World Negroes. As he points out, the anthropologist may take the culture of the West African Negro groups, from which the New World Negroes originally came, as a "baseline" or kind of control group. To this "baseline" he may refer New World groups differing with respect to such factors as the conditions of servitude under which they were brought to the New World, the dominant social, political, economic and religious patterns of the European group with which they came in contact, etc. As more groups are studied it is possible to narrow down, by comparisons within and between Negro groups, a number of crucial determinants of the course and direction of the acculturation process.

Cross-cultural studies also contribute to the determination of pertinent relationships, although at present their primary purpose appears to be the discovery of universal relationships that exist among observed phenomena rather than the determination of causal factors. The observation of relationships through the total range of known cultures and the examination of extremes in degree

of relationship point the way for further analysis of causal factors.

An example of such cross-cultural research is Murdoch's study of the relationship of matrilinear and patrilinear societies to the advancement of civilization (15). Murdoch examined a sample of 230 cultures representing approximately equal numbers of tribes from all regions of the world. He selected the same number of tribes from different parts of each culture area. A few samples of higher cultures were taken to make the total representative of the whole world. Matrilinear and patrilinear societies were analyzed into specific component traits and the advancement of civilization was precisely defined in terms of social organization and economy. Since the variables were analyzed in terms of specific traits, it was possible to observe certain matrilinear traits, for example, which were associated with less advanced civilization though there was no evidence for greater association of matrilinear societies, taken as a whole, than for patrilinear societies.

The validity of applying the findings of this study to the entire world rests on the representativeness of the sample tested. The validity of the analysis of traits in each culture depends, in turn, on the validity of the original ethnographic reports. The ethnographer, though he does not often achieve a truly representative sample, nevertheless attempts to observe and obtain information from individuals who represent different ages and sexes and different sub-groups within the culture studied. To the extent to which he approximates a representative sample, his findings gain in validity for the specific group tested, and pave the way for precise manipulation of variables in order to determine pertinent relationships.

C. Techniques related to dependability or reliability. The ideal of de-

pendability, like that of pertinency, is difficult to attain because cultural and individual conditions cannot often be closely reproduced. The ethnographer must therefore observe a given event, custom or rule as many times as possible under similar conditions, or the same event or custom as described by as many informants as possible. Similarly he must note a particular type of behavior as manifested in as large and as homogeneous a group of individuals as possible or as seen in the same individuals at different times under similar conditions.

Though ethnographers often fail to state in their published research the number of observations made or the number of informants who have reported a particular event, it is understood that no responsible investigator reports a rule or norm based on a single observation or on the report of a single informant. Malinowski, for example, cautions the ethnographer to "exhaust as far as possible all the cases within reach" (12), and Mead emphasizes the need for taking enough of the same situation with similar individuals, or the same individual at different times, to get quantitative statements (14).

Ford (3, pp. 155-156) describes a method for recording the reliability of rules and patterns in a society both with regard to (1) the number of individuals affected by the rules in proportion to the total population, and (2) the number of circumstances when these individuals are affected throughout their lives. He suggests a quantified rating for each of these ranging from "one to a very few members" to "most to all members" affected, and for circumstances when affected during lifetime, "seldom to often" to "all the time." Where an adequate sample is observed, this scale can be used for report of frequency of rules and norms within the culture or for cross-cultural comparison of specific rules and norms.

It also allows the ethnographer to present quantitative measures for reliability which may be subjected to the usual tests for statistical significance, or at least to describe variability in terms of ranges or overlapping distributions.

Where quantitative determinations of frequency are not made, the ethnographer customarily gives some indication of the reliability or generality of the patterns described by reporting deviant as well as "normal" cases. Whiting describes a special criterion to be used in participant observation to discover whether an act is customary or deviant. If it is customary, there will be no critical comment from the observers; if it is deviant, the observers' comments will reveal it (21, p. xvii).

In a forthcoming study Whiting and Child discuss the problem of reliability in a cross-cultural study where the degree of intensity of a custom or trait is determined by judges' ratings made from the ethnographic data. They discuss statistical tests for the reliability of judges' ratings, the need for setting up a standard for reliability before ratings are made and the loss in reliability resulting from the use of too general categories or from ratings made by a single individual rather than several judges. The method of obtaining reliable results for their data is illustrated in this study (22).

III. CONCLUSIONS AND SUMMARY

This survey of anthropological methods from the scientific viewpoint reveals a somewhat more promising picture than might be expected in view of our earlier analysis of the studies reported in an anthropological journal. It is evident that much of the work of cultural anthropologists is carried out with as close compliance as possible with the requirements of scientific research. The obstacles to increasing the scientific value of anthropological data ap-

pear to come, not from a lack of ingenuity or interest in devising scientific techniques, but rather from a failure on the part of many anthropologists to adopt standards and methods already suggested. This resistance to the adoption of uniform standards appears to exist either because the anthropologist is ignorant of the suggestions which have been made, or because he disagrees with the specific suggestions made, or because he is convinced that cultural data can not yet be scientifically studied and reported.

It is not suggested here that the methods and techniques discussed exhaust the possibilities of scientific method in anthropology, nor that the data obtained from a particular culture can be completely observed and reported by these means. Just as the psychologist in designing an experiment suits his techniques to his problem or supplements quantitative analysis by qualitative analysis of subjective data, so the anthropologist in the field chooses the techniques best suited for his purpose and adapts them to the culture under observation. He also, to a greater extent than the psychologist, supplements quantitative data by qualitative description and illustration. If anthropological data are to lead to scientific prediction, however, it is essential, first, that limited problems be designed to test specific hypotheses wherever this is possible; and, second, that certain standard procedures and methods of observing and reporting be agreed upon at least within a certain area of research. The field worker who finds it necessary to devise special techniques should satisfy himself that these procedures will stand scientific scrutiny.

This paper has been presented specifically for the psychologist who is interested in working toward the integration of the social sciences, but who finds such a great gap between his own laboratory methods and the field tech-

niques of the cultural anthropologist that he is unable to accept the findings of anthropological studies. It may be argued that this gap is more apparent than real, for it results first, from failure on the part of the psychologist to consider the present status of anthropological method in its historical perspective, and second, from the psychologists' ignorance of anthropological research beyond the short reports appearing in the current journals. Accordingly, we discussed briefly the present stage of development of anthropological method and considered its present status as it appears to the casual reader of anthropological journals. We then attempted to go beyond this picture of anthropological research techniques and to relate to scientific criteria some of the methods and practices which are used or have been suggested for use by anthropologists who are interested in increasing the scientific value of their data.

Such a survey was presented in the hope that even a brief presentation of anthropological methods in relation to scientific criteria would provide a better basis for the psychologists' evaluation and understanding of the methods and findings of cultural anthropology. On the basis of such understanding the psychologist, out of his longer experience in the manipulation of the variables of human behavior, may be able to suggest some of his particular techniques which might be adapted for use in anthropological field studies.

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DYNAMIC SYSTEMS AS OPEN NEUROLOGICAL SYSTEMS

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This is the third in a series of papers devoted to an exposition of the Dynamic Systems theory. In the first of these papers (10) I attempted a general programmatic statement of the theory. In the second paper (11) I presented a first specification of the theory, gave my reasons for taking the position that the Dynamic System (or any hypothetical construct in psychology) must be thought of in purely neurological terms, and defined the Dynamic System as the functional unit of the brain field. The objective of the present paper is to specify three general attributes of Dynamic Systems: (a) the pattern of activities which are presumed to exist in a Dynamic System, (b) the significance of describing the Dynamic System as an open one, and (c) the locus of Dynamic Systems.

Before taking up these three questions, however, it may prove profitable to make a few general observations about the nature of neurological speculations when indulged in by psychological theory-builders. It is my hope that if I can make explicit the principles which I shall attempt to follow when making my speculations, then the area of probable misunderstandings which will be evoked by these speculations will be decreased.

ON NEUROLOGICAL SPECULATIONS

Perhaps the first guiding principle which one should set down for the psychologist intent on neurological speculations is that he should pay proper respect to present neurological knowledge and theory. With such an obvious statement I would agree, but

only if I were permitted to specify the meaning of "proper respect." "Paying proper respect" is not the same as "being subservient to." That is to say, I would not interpret the above guiding principle to mean that the psychologist must not venture into territory which the neurologist has not yet explored, nor that he must be reluctant to disagree with what the neurologist has already determined. The reasons why I insist on this permissiveness are, I believe, crucial to an understanding of the proper relationship which must be worked out between neurology and psychology.

Essentially we are faced with the problem, when we try to combine psychology and neurology, of either *trimming down our psychology* to the possibilities inherent in present-day neurology, or else *expanding neurology* (even if only by piling speculation upon speculation) to encompass what we already know about psychology. It is the former which has been done in the past and it is that procedure which has resulted in an understandable impatience with "neurologizing" among psychologists. Thus MacLeod (17) points out that previous attempts "... tended to restrict psychological investigations to just those processes for which simple physiological counterparts could be found, and by implication to brush aside as of secondary importance the very phenomena which originally inspired curiosity. . . . The psychologist in his desire to be accepted in the fraternity of the natural scientists almost lost sight of his original objective. The ultimate problems of cognition

were becoming gradually obscured by an ever thickening veil of sense-receptors and nerve-fibers." Such sterilization of psychology is inevitable, in my opinion, so long as we approach the problems of the neural bases of behavior as *neurologists* rather than as psychologists. We must be psychologists first and neurologists second. But, and this is my major contention, if we are psychologists first we will, by that very token, become *better neurologists*. To build up neurological hypothetical constructs for psychological theory must not involve, at any point, a denial of, or compromise with the data of behavior and experience. *It is the psychological data, in the last analysis, which must provide the tests of the adequacy of any theory of brain action.* And we will find, I believe, that if we go about our neurological speculations in this spirit, that the most cherished principles of present-day neurology will have to be re-examined and overhauled and the entire field of neurology redefined. There is another point to stress in this connection. The unity of science will not be achieved by *reducing* psychological principles to neurological ones, and neurological ones to physical ones. What we must seek is to make physical principles *congruent* with neurological ones, neurological ones with psychological ones, and at each point the most inclusive set of data must be the test of how well we achieve this congruence. Again, this means that we must test the adequacy of our physical principles by how well they can encompass neurological data, and neurological principles by how well they can encompass psychological data. As we go from the data of one scientific field to another in our search for unity, we should not seek to *reduce* the data, but we should seek to *redefine* the principles so that borderlines begin to disappear. I will,

later in this paper (see page 351), make this point with specific reference to the relationship between biological and physical principles, but the general position I am taking here is that *the more inclusive field must enlarge the concepts of the less inclusive if we are ever to achieve a genuine unity of science.* It is because of all of the above considerations that I insist on the psychologist's right to differ from present-day neurological theory.

But if all of the above tells us what I do not mean by "paying proper respect to present neurological knowledge and theory," what is it that I do mean? First I would assert that the speculating neurological psychologist is not absolved from the responsibility of knowing the neurologist's work and theory. Whenever we make neurological statements we must examine them in the light of current neurological thought. If our statements violate present theory we must show awareness of that fact, make the disagreements explicit, and justify our preference. Another thing I do mean by "paying proper respect" is that, everything else being equal, I prefer those speculations which do the least violence to orthodox neurological theory. But the phrase "everything else being equal" must be kept constantly in mind and refers to the usefulness of the speculations for psychological theory, *i.e.*, for their ability to handle molar behavior and conscious experience. Finally, I subscribe to the principle that, everything else being equal, I prefer those neurological hypotheses which are most amenable to direct empirical verification, given our present research techniques. The reason for this is that the attributes we give to our neurological hypothetical constructs must be open to eventual direct study—and the sooner, the better. So much, then, for our guiding principle

that our neurological theorizing should pay proper respect to present neurological knowledge and theory.

DYNAMICS SYSTEMS—CIRCUITS OR FIELDS?

The first question we now want to consider is whether the Dynamic System, as a molar unit of the brain field, can be described functionally as consisting of a complex series of *neuron-neuron* coupled activities or whether it consists of a *continuous field* of activities. A more general way of stating the question is to pose it as the problem of the *patterning* of activities in the Dynamic System.

The neuron-neuron hypothesis would assert that the pattern of activities in a Dynamic System could be adequately described as consisting of "longitudinal sequences." According to this formulation the major activity of the nervous system depends upon a chain of neuron-neuron firings wherein one set of neurons after another is thrown into action in longitudinal sequence. This description is, of course, the classical one, the one which has most common acceptance, and the one which has much supporting evidence. It should be clear, however, that the phrase "longitudinal sequence" does not necessarily imply that the brain merely acts as a transmission center from periphery to periphery. In recent years brain models have been suggested which, while faithful to the longitudinal sequence description, make it possible for us to conceive of continuing neural activity in the brain even after the cessation of the initiating neural condition. The basic principle behind such models is the closed neural circuit, *i.e.*, the assumption that neurons may be so arranged in a circular fashion as to permit impulses to travel repeatedly around the closed chain.

Köhler, however, has protested that

the longitudinal sequence pattern permits but an inadequate description of neural activity. Believing that "... many activities of the nervous system are relationally determined in a way which we cannot understand in terms of separate actions within anatomical elements" Köhler has suggested that apart from the events within chains of neurons, "... specific processes (electrical) spread across the tissue as a continuum when conditions in one part of the tissue become sufficiently different from those in an adjacent part" (9). To support this proposal Köhler calls upon psychological and neurological data. His psychological evidence is based primarily upon his and Wallach's well-known study of the figural after-effect (9). His neurological evidence is taken from Gerard and Libet's work which seems to indicate that parts of the brain can remain functionally interrelated even when all connecting fibers have been severed, or when their synapses are blocked by drug action. Recently Köhler and Held (8) have presented data which seem to indicate the presence of direct currents in the brain—a phenomenon which might be interpreted to support the assumption that the brain acts as a volume conductor. In addition it should be mentioned that the demonstrations that neural cells can be stimulated to fire by the nutrient fluids bathing them (see Hebb's [5] summary of these data) support the view that neuron-neuron induced firings is an inadequate description of brain action.

We are thus presented with two choices for a postulated pattern of activities in the Dynamic System. It is clear, of course, that these are not completely different patterns since the field pattern can include the longitudinal one. The functioning of a closed neural circuit, for example, could well be conceived of as a part-process of

Köhler's neural field. Insofar as there is a difference, present neurological evidence and theory do not clearly support either one in preference to the other. Either one, it seems to me, could be well defended on physiological grounds alone. This consideration by itself would predispose me to prefer the field pattern over the neuron-neuron pattern since I believe it to be better practice to take the less restrictive position at this stage of neurological knowledge—a stage which is so speculative and fluid. In addition, there are psychological considerations which also predispose me toward a preference for the field description. As I hope to show at some later time, the assumption of a field pattern makes the Dynamic System a more amenable hypothetical construct for dealing with various systematic and "autonomous" changes which seem to characterize behavior and experience. Because, then, present physiological facts permit it, and psychological considerations seem to favor it, I shall adopt the field description of the patterning of neural activity in the Dynamic System. However, I shall keep most of the attributes of neuron-neuron coupling activity. This latter I do, again, because of both physiological data and psychological implications. I now want to present a summary statement of my first postulated attribute of Dynamic Systems. This statement derives from the considerations mentioned above and attempts to describe the *activity pattern* of the Dynamic System:

The Dynamic System is to be conceived of as a field of electrical-chemical activity. This field of activity, however, does not occur in a functionally or anatomically undifferentiated homogeneous matrix. The substratum for this field is articulated and can be described as consisting of sub-units and primary loci of activity (neuron-chains,

synapses, etc.) as well as intervening tissue which act as conductors. The neuron-chains and synapses can be thought of as local¹ organizing sub-structures of the entire activity field.

DYNAMIC SYSTEMS AND THE SECOND LAW OF THERMODYNAMICS

In my first definition of Dynamic Systems I characterized them as "open" systems, rather than as "closed" ones. An open system is defined as one which shows a constant import and export of material and energy with the outside world. A closed system is one which does not show any such exchange—nothing enters the system, nothing leaves it. This difference between system-environment relationships leads to differences in explanatory theory and in specific attributes of the two kinds of systems. It is these differences which we must now consider.

In our previous discussion it was pointed out that "The Dynamic System is to be conceived of as a field of electrical-chemical activity." This might be interpreted to mean that the ordinary laws of physics as applied to electrical-chemical fields would suffice for a full description of the Dynamic System. But such an implication is not warranted. The Dynamic System is not only an electrical-chemical field, but it is also a biological system, *i.e.*, an open system—and many of the laws of physics simply do not apply to open systems. This is especially true of the second principle of thermodynamics² and the laws of physics derived from that principle, for it is clear that this

¹ The differentiation between "local" organizing factors and more distant organizing factors will be made later, see pages 357-358.

² It will be remembered that the second principle states that the entropy of a closed system can never decrease, *i.e.*, that insofar as there is a change in the "quality" of energy, there is general tendency toward a degradation of energy, *i.e.*, final equilibrium.

second principle applies specifically and only to closed systems. This, in turn, raises a major scientific question and many specific problems about the behavior of systems. Both sets of problems have immediate relevancy to our discussion of the attributes of Dynamic Systems.

The major scientific question which is raised is this: If the laws of thermodynamics do not apply to Dynamic Systems (and all other biological systems) does that mean we must seek extra-physical laws for understanding the behavior of Dynamic Systems? Does this imply, in other words, some sort of vitalism? This problem has been stated more dramatically as follows by the theoretical physicist, Brillouin (2): "How is it possible to understand life, when the whole world is ruled by such a law as the second principle of thermodynamics, which points toward death and annihilation?"

Schrödinger (18), another noted physicist, formulates a possible answer to that question by asserting that, "We cannot expect that the 'laws of physics' derived from it [the second principle of thermodynamics] suffice straightaway to explain the behavior of living matter. We must be prepared to find a new type of physical law prevailing in it. Or are we to term it a non-physical, not to say a super-physical law?"

There is one compelling reason why we are immediately concerned with this knotty and seemingly abstruse problem. Since I have defined a Dynamic System in terms of electrical-chemical fields I must be clear about the properties of that field (unless the term "electrical-chemical field" is to be used merely as a "helpful analogy"). The two major characteristics of a field which will concern us most are those of "reorganization" and "equilibrium." The second principle of thermodynamics has a number of specific things to say

about these two attributes. Among other generalizations, the second principle of thermodynamics demands that (1) the history of field reorganization in any given system be characterized by spontaneous progressive simplification and increase in homogeneity, and (2) equilibrium, or final stability, be characterized by the cessation of all activity and capacity for work. To postulate different attributes for biological systems (and in some cases opposite ones) would be, to many, either an assertion of the existence of an insuperable barrier between the physical and biological, or even a confession of *ad hoc* vitalistic thinking. And yet, from everything we know about the behavior of biological systems (among which must be the Dynamic System) it is demanded that we make such postulations. I am then faced with the problem of whether, by postulating attributes to Dynamic Systems which run counter to the attributes of physical systems (and I will, shortly, make such postulates) I am either denying the ultimate unity of science or else am endowing my Dynamic System with attributes which no electrical-chemical field under heaven can ever have.

There is another reason why I believe that this whole discussion is of interest to us in connection with psychological-neurological theorising. Some of psychology's most ambitious and promising attempts at model-building for brain action have been carried out under the implicit assumption that our models for electrical-field action in the brain must obey the laws of physics which apply to other electrical fields. This assumption, because it did not recognize adequately that the brain field is a biological field is, in my opinion, an erroneous one and has led to both errors of omission and commission. The Gestaltists' models are per-

haps the best illustrations of what I have in mind in this connection.

The Gestaltists rejected vitalism, but they observed certain psychological phenomena which *seemed* to be inconsistent with our usual notions about the behavior of physical objects. One of Köhler's most brilliant accomplishments was to demonstrate that this inconsistency was in large measure only an apparent one, and not a real one. To do this, Köhler worked with electrical fields as his medium—probably choosing this particular field of inquiry because brain processes are electrical in nature. Among the phenomena which he was able to demonstrate in electrical fields were: transposability, relational determination, subordination of parts to the whole, and the tendency toward maximal simplicity. While this was a significant step toward the goal of removing the barriers between the organic and the inorganic, such demonstrations, if they are intended to serve as guides toward principles of brain action, are inadequate. In the first place certain phenomena or principles which are true of a biological field simply are not to be found in non-biological fields (for reasons already mentioned and for additional reasons which will become clear in our later discussion). In the second place, certain phenomena which are found in non-biological fields are reversed in biological fields.³ To avoid these errors, Köhler would have had to go to biological systems for his "isomorphs" and not to non-biological electrical fields. But then the very same question would arise which we have posed before: If we use a set of "biological laws" in order to justify attributing certain

characteristics to electrical fields in the brain, would we not be departing from a purely physical demonstration of "form," "*Gestalten*," etc.? Would we not, in other words, be denying the unity of science by implying that these phenomena are the peculiar attributes of living systems only?

It seems to me that the best answer to this question is the one proposed recently by both Bertalanffy (1) and Brillouin (2). Both of these writers, one a biologist and the other a physicist, first point out that we must be clear about the limitations as well as the validity of our laws of physics. Thus Brillouin writes, with respect to the second principle of thermodynamics: "Nobody can doubt the validity of the second principle. . . . However, the question is to specify its domain of applicability and the . . . type of problems for which it works safely," and then he points out "we can easily find many cases where it is useless and remains dumb."⁴ The one major limitation of the second principle, as has already been indicated, is that it applies only to closed systems. This means, of course, that almost *any* attribute which one may postulate for an open system is perfectly permissible as far as the second principle of thermodynamics is concerned. But does that mean that it is in the open system that a new "vital" principle enters? Not at all, replies Bertalanffy. All that this need mean is that, since our laws of thermodynamics are based on the observations of a limited set of data (closed systems only) what is required is a more inclusive "physics" and the development of a "General System Theory" in which closed or open systems are seen as special cases. Berta-

³ Among such instances may be the so-called principle of simplicity. Some of the reasons why I question this basic principle of Gestalt psychology will be indicated later in this discussion. See page 353.

⁴ It should be pointed out that Brillouin indicates that the second principle is useless not only for open systems, but even for many purely physical (*i.e.*, non-biological) systems.

lanffy quotes Prigogine to the effect that:

... classical thermodynamics is an admirable but *fragmentary* doctrine. This fragmentary character results from the fact that it is applicable only to states of equilibrium in closed systems. It is necessary, therefore, to establish a broader theory, comprising states of non-equilibrium as well as those of equilibrium.

Bertalanffy further points out that the development of such a broader theory (especially at the hands of Prigogine has been begun and has already indicated the nature of certain attributes which distinguish an open from a closed system. Further, claims Bertalanffy,

In physics, the theory of open systems leads to fundamentally new principles. It is indeed the more general theory, the restriction of kinetics and thermodynamics to closed systems concerning only a special case.

What he is suggesting, in other words, is that *attention to biological phenomena will revise and improve our theorizing in physics*. This "reorganization of the field" in the thinking about the relation of physics to biology is indeed a provocative one. Its possible fruitfulness is concurred in by Brillouin who illustrates this point with a compelling example—one which merits quoting in full:

During the nineteenth century physicists were desperately attempting to discover some mechanical models to explain the laws of electromagnetism and the properties of light. Maxwell reversed the discussion and offered an ... electromagnetic interpretation of mechanical properties of matter. We have been looking, up to now, for a physico-chemical interpretation of life. It may well happen that the discovery of new laws and of some new principles in biology could result in a broad redefinition of our present laws of physics

and chemistry, and produce a complete change in point of view.

To restrict ourselves, then, to brain-action theories derived from current physics may not only lead us into error, but we run the risk of all this without even achieving grace. That is, if our aim is to demonstrate that nothing in open systems is incompatible with the laws of physics, then we may well be on the wrong track when we try to seek for isomorphisms between biological and "purely physical" systems. Our *physics* might eventually prove to be more accurate by our being biologists than by attempting to be "pure physicists." Or, to put it still another way, to see differences between biological systems and "purely physical" systems is not to assert, implicitly, that a vitalistic principle is involved in the former, but, rather, it is to suggest that the latter must be expanded to become a better physics.

Dynamic Systems, then, show all the characteristics of open systems and there is nothing incompatible between such a position and the position that all science is one. But what does this mean as far as the specific attributes of Dynamic Systems are concerned? Here I will base my answer on some of the suggestions of Prigogine as presented by Bertalanffy. There are two major attributes of open systems which I wish to consider. The first refers to the reorganization process, and the second, to the nature of a "steady state." Some of the characteristics of both of these seem to be determined by Prigogine's formulation that the total change of entropy in an open system can be written as follows: $dS = deS + diS$, where dS denotes the total change of entropy for the system, deS , the change of entropy by import, and diS , the production of entropy due to irreversible processes in the system (e.g., chemical

reactions, heat transport, etc.). Now $d_i S$ is always positive, while $d_e S$ may be either positive or negative. Therefore the total change of entropy in an open system can be either positive or negative. This means, among other things, that (1) in open systems entropy may decrease and therefore such systems may spontaneously reorganize toward states of greater heterogeneity and complexity, and (2) an open system may remain in a steady state (*i.e.*, where the system remains constant as a whole and in its phases, though there is a continuous flow of the component material) and therefore a system in its "final" organization can continue to do work.

I now want to state the above considerations in a somewhat more formal set of postulated attributes of Dynamic Systems, and then present a few brief comments about these attributes. I have based the formulation of these attributes on the considerations presented by Bertalanffy and inadequately summarized in the preceding paragraph. But this does not imply that I believe that these attributes follow rigorously from any demonstrably adequate thermodynamic theory of open systems. Quite frankly I have chosen these attributes just as much because I will find them useful, later, in dealing with psychological data as because I am convinced by Prigogine's derivations. But at the same time I believe that (a) the following postulated attributes of Dynamic Systems do not violate present physical laws relating to fields of electrical-chemical activities, and (b) may later be shown to be congruent with such laws.

Dynamic Systems may develop spontaneously toward states of greater heterogeneity and complexity.

A Dynamic System, in its final reorganization, may maintain a steady state and continue to do work.

Now for a few brief comments about the above attributes of Dynamic Systems. First of all it must be clearly understood that they are not presented as "laws" but as general statements of "possibilities." Neither one of the above statements details the conditions under which certain specified events may occur. Thus, for example, the first statement says that Dynamic Systems *may* develop toward states of greater complexity, but it does not indicate the conditions under which Dynamic Systems will do so. These are specifications which must wait upon further work.

The term "spontaneous" in the first of the above statements needs some clarification. As I intend to use it, I mean by spontaneous "without peripherally induced stimulation," *i.e.*, not dependent upon any sensory stimulation. This does not mean, of course, that such reorganization cannot take place as a consequence of sensory stimulation. Another implication of the above statement is that Dynamic Systems may combine with other Dynamic Systems into a higher-order organization which I shall call "Dynamic System Families." A higher-order organization (one involving several Dynamic Systems) will be, of course, more heterogeneous and more complex than any of its constituent systems. The tendency of Dynamic Systems to combine with other Dynamic Systems will be the subject for a fuller treatment in a later paper, and I mention it here only because I wish to clarify the meaning of this postulated attribute of Dynamic Systems and also because I will make some use of Dynamic System Families in my discussion of the problem of brain localization (see pages 353-360 of this paper). However, it should be pointed out that when one Dynamic System combines with another to form a Dynamic System Family the activi-

ties *within* that specific Dynamic System are now determined not only by what is going on locally (*i.e.*, within that Dynamic System) but also by what is happening to the other members of the Dynamic System Family. In other words such higher-order organizations cannot be considered as associations of *independent* systems.

While it is not my intention, in this paper, to discuss the behavioral consequences of these neurological speculations I might anticipate some of my future discussions by calling attention to at least one of the implications of the first of the above attributes. This implication is concerned with the "Law of Pregnancy" as formulated by the Gestaltists. That law, as phrased by Koffka (6) states that "psychological organization will always be as good as the controlling circumstances permit," and "good" includes such characteristics as regularity, symmetry, maximal simplicity, etc. The notion that all experienced forms tend toward regularity, symmetry and maximal simplicity seems to be derived from argument by analogy with the behavior of purely physical systems and from the course of events which takes place as described by the second principle of thermodynamics. Our postulated attribute of Dynamic Systems, on the other hand, suggests that some experienced forms, under some circumstances, may tend spontaneously toward *increased* heterogeneity and *increased* complexity. It will be shown (in a later paper) that a critical re-examination of the psychological data justifies considerable doubt as to the universal applicability of the Law of Pregnancy. I must hasten to confess that until such a re-examination has been presented these comments can be justly accused of being gratuitous. However, my purpose in making these anticipatory comments is merely to point up the fact that our present

postulate has consequences which are different from those of the "purely physical" ones and can therefore generate predictions which should help us to decide between them.

As far as the second postulated attribute is concerned, only a few points need be made at this time. In the first place, the word "final" is intended to refer to the life-span of the organism. That is, it is assumed that a Dynamic System can undergo reorganization and then, under certain conditions, cease from all further change but continue in that unchanged condition throughout the life of the individual. The second point is that a Dynamic System in such a state (*i.e.*, when not undergoing reorganization) can still do work, that is, can still influence behavior. That, of course, would be impossible for a system in equilibrium. A system in equilibrium, by very definition, is one where entropy is maximal and no energy is available for work. Finally, it should also be pointed out, that for a Dynamic System to remain in a steady state implies that a Dynamic System manifests forces which are directed against a disturbance of its steady state.

DYNAMIC SYSTEMS AND BRAIN LOCALIZATION

The Dynamic System has thus far been defined as an open system of electrical-chemical activity occurring someplace in the brain. But it is clear that Dynamic Systems are not all momentary affairs, existing only as immediate responses to stimulation and disappearing after the stimulation has been withdrawn. As our major hypothetical construct, which is to explain all of behavior and all of experience, the Dynamic System must be conceived of as being relatively enduring—in some cases, as we have seen, enduring in an unchanged state for many years. This

means that we must deal with the very important problem of brain localization. Where, in the brain, do these enduring open systems of electrical-chemical activities take place? For various reasons (some of which are obvious from our previous assertions about the attributes of Dynamic Systems) I find it desirable to assume that Dynamic Systems are permanently located within specific, spatially-defined loci in the brain. I am subscribing, in other words, to the localization hypothesis. Such a position, however, in the light of most neural-behavioral studies which argue against the localization hypothesis, requires justification and clarification. It is to this problem which we now turn. Before considering the possibility of maintaining a localization hypothesis in the light of current evidence it is necessary to make clear what is meant by the term "localization hypothesis." That hypothesis can mean quite a number of different things. Let us specify two general interpretations which can be made—one, the usual interpretation and the other, the one which I shall adhere to.

As normally used, the term "Localization hypothesis" refers to three assumptions: (1) that different behavior *functions* have their controlling neural mechanisms in specified anatomical areas of the brain, (2) that the neural mechanism for any one type of behavior function will be found in the same anatomical brain area for different individuals of the same species, and (3) that the going-off of any specific type of behavior function is determined only by the events occurring within the appropriate anatomical brain area. Thus, for example, it is assumed that there is a specific neural mechanism which controls the "speech" function, and that such a neural mechanism is found localized in the middle regions

of the dominant hemisphere of all human beings, and that destruction of that limited area will destroy the ability to speak.

As I have already indicated, the above usual meaning is not the only possible interpretation of the localization hypothesis, nor the one which I shall use. The localization hypothesis can be interpreted in a much more general way as follows: (1) Different behavior *units* have their controlling neural mechanisms in different anatomical areas of the brain, (2) the neural mechanism for any one unit of behavior may be found in different anatomical brain areas for different individuals of the same species, and (3) the going-off of any behavior unit is determined by the functioning of the whole brain as reflected in the events occurring within the appropriate anatomical brain area.

The following general considerations suggest that the first of the above two interpretations is inconsistent with some of the basic concepts behind the whole theory of Dynamic Systems. It must be recalled that in my first paper of this series (10) I questioned the validity of conceiving of experience (or behavior) as made up of separate elements or units of "motivational" experience, "perceptual" experience, "cognitive" experience, etc. I suggested that we must conceive of our fundamental unit of experience (or behavior) as a motivational-perceptual-cognitive *unit*—no matter how trivial and momentary or how important and enduring that experience might be. Accordingly, in my second paper (11) I defined Dynamic Systems (the hypothetical constructs to which such unified experiences were coordinated) as "... molar organizations of specified neural events persisting from previous brain activity and of specified neural events deriving from stimulation origi-

nating outside the organism or inside the organism." This means that Dynamic Systems, as *molar organizations*, were not to be analyzed into bundles of independent traces of "internal" and "external" events, or of "sensory" and "associative" ones, or even of "present" and "mnemonic" ones, but rather that the whole complex of neural activity was to be considered as a unified and unitary field. From this point of view there cannot be a different "type" of Dynamic System for each different "type" of behavior function. For example, we could not suppose that there is one Dynamic System coordinated with the ability to *perceive* the word "wife," another Dynamic System, to *speak* the word "wife," another one to *cognize* the meaning of the word, and still another one to *evoke an emotional response* to the word. To conceive of such partial or fragmentary Dynamic Systems would be to deny the very essence of the theory. Since, then, there are no different "types" of Dynamic Systems it would be a meaningless question to ask whether different types of Dynamic Systems were localized in the same brain areas for different individuals. In other words, the traditional analysis of behavior into functional atoms (perceptual, cognitive, or emotional) is essential to the hypothesis that different types of behavior functions have their neurological determinants in different areas of the brain, and therefore that hypothesis is not consistent with our theoretical formulation.

The above considerations alone would be enough to warrant my rejection of this formulation of brain localization theory. But quite aside from these considerations, the available empirical data also permit (even if they do not demand) such a rejection. There are few workers in the field—clinicians or experimenters—who would assert that

brain lesion data support the hypothesis of specific brain localization of functions as demanded by the first hypothesis. For every datum which can be cited to support functional localization in specific brain areas, another equally compelling datum can be found which contradicts this interpretation. Among the latter data are, of course, Lashley's monumental neural-behavioral studies.

The second interpretation that I have suggested does not have the basic difficulty for Dynamic System theory which the first one has. The second interpretation, it will be recalled, was to the effect that any given Dynamic System could be permanently localized in any part of the brain and that the specific locus of this brain area could differ from individual to individual even though the unit of behavior in question was quite similar for the different individuals. Because such an interpretation does not necessitate the notion of fragmentary behavior functions nor "types" of Dynamic Systems, we are not under obligation to seek for "speech" centers or "higher mental function" centers, etc. There is no necessary contradiction, in other words, between our concept of unified behavior units, unified neural units, and brain localization of this type. However, such negative virtues are not enough. We are still faced with the problem of the neural-behavioral studies which seem to assert that there is no evidence for any kind of localization, but, rather, that the brain operates as a dynamic whole.

Perhaps the best way to indicate how our proposed formulation of a localization hypothesis could deal with such data is to consider some of Lashley's experimental work. Basic to Lashley's experimental design was the assumption that the neural foundations for any learned act (such as running

a specific maze) was localized in the same brain areas for all of his animals. Starting from this assumption it was logical for Lashley to look for a *common* crucial area, among his operated animals, the destruction of which would interfere with the running of the maze. Since Lashley found no such crucial area, he argued against the whole concept of brain localization and proposed, instead, the hypothesis of equipotentiality and over-all brain functioning. But his failure to find a common crucial area can be interpreted to mean that only the first description of the brain localization hypothesis does not hold. As far as the second interpretation is concerned there need be no contradiction. His method of analysis was not appropriate for checking upon that hypothesis. However, there are his positive findings which indicate a correlation between the amount of cortical tissue destroyed and extent of behavioral interference of a learned act. This requires further consideration.

To reconcile Lashley's positive results with our variant of the brain localization hypothesis I first want to call upon the concept of Dynamic System Families. It will be remembered that I have already assumed that Dynamic Systems tend to reorganize with other Dynamic Systems. This would certainly happen in the course of the establishment of so complicated a sequence of behavior as running a maze for food. It was further assumed that the individual members of a Dynamic System Family are so interrelated that the activities within one are affected by, and affect, the activities in all the other members. This means that coordinated with the behavior of running a maze are the interrelated activities among a *number* of Dynamic Systems. These different Dynamic Systems need not be, of course, in close spatial proximity. It is only necessary that they

be functionally related.⁵ All we now have to assume is that the larger the area of cortical destruction, the greater is the probability that the pattern of activities among the members of the Dynamic System Family will be altered. Small invasions of individual Dynamic Systems could, conceivably, have no measurable effect simply because one of the properties of an open system is that such systems manifest forces which are directed against a disturbance of their steady states. (See page 353.)

It would seem then that the above "probability" analysis makes tenable a localization theory even in the light of Lashley's results. This is, of course, the type of analysis which has been advanced by others, among such attempts being those of Köhler (7). However, this reconciliation is inadequate as a complete story of brain action if for no other reason than the kind of observations recorded by Goldstein (4) concerning the effects of brain lesions on human behavior. It will be remembered that he points out that quite aside from any specific symptoms resulting from localized brain injury, there seems to be an over-all generalized symptom characteristic of brain-injured patients—disturbance of the so-called "categorical behavior," for example. The observation of a generalized effect of brain injury has been made by other clinicians as well. A number of years ago I presented some data which indicated the same sort of effect in brain-injured rats (14, 15, 16). Among the conclusions which I came to at that time were the following:

⁵ In 1935 I presented some experimental evidence (13) which indicated that certain behavior in the rat was a complex function of the neural activities which took place within two anatomically delimited (and spatially separated) cortical areas. Also at that time I proposed a theoretical explanation similar to the one outlined above.

A possible effect of a cerebral lesion is the lowering of the 'level of attention' or 'vigilance' (to use Head's term) of the animal. This deficiency in attention may account for the observed loss in general capacity of the brain injured animal. In addition, an animal with even a minor lesion can show various *qualitative* changes in behavior such as a loss in the ability to adopt generalized responses, a loss in the variety and plasticity of his hypotheses, and, finally, something very much akin to some of Goldstein's descriptions—a loss in the ability to use *umweg* procedures in problem solving.

It seems highly unlikely that the effect of a brain injury is adequately described by stating it as the sum of a large number of specific interferences with discrete traces (Köhler) or units of behavior. Both clinical and experimental results from work with human beings and with rats argue strongly for some *general* disturbance in brain function as a consequence of cortical insult. How then can a "general function" be congruent with a localization hypothesis, and does not the admission of such general functions argue against our Dynamic System theory with its analysis of behavior into all-inclusive units rather than into independent (or even interacting) *functions*.

A consideration of the attributes of Dynamic Systems, it seems to me, provides a number of possible ways to meet the above problem. Dynamic Systems are open systems. As open systems they are in constant communication with their environment, and they are amenable to "independent" reorganization and to reorganization with other Dynamic Systems. There are three implications of this which I wish now to consider: (1) there is a functional intercommunication among all Dynamic Systems, (2) All Dynamic Systems have common dependence upon the sensory projection areas of

the cortex, and (3) all Dynamic Systems share a substrate which can be characterized by a common physiological state. Let us see what each of these contributes to the solution of our present problem.

The postulated ability of Dynamic Systems to form Dynamic System Families must mean that some degree of communication is open between any one Dynamic System and *every other Dynamic System*. That is, a Dynamic System is not only in functional contact with the other members of its Family, but, to some degree, with every conceivable Dynamic System in the brain. To suppose otherwise would make it difficult to see how new Dynamic System Families could ever come into being or how old ones could ever be changed or enlarged. To say that every Dynamic System is in functional contact with every other Dynamic System is to say that every Dynamic System is in contact with events occurring *any-place* in the brain. In other words there is a hierarchy of organizing factors for any one Dynamic System: First there are the *specific local* organizing factors (e.g., activities in the neuron chains of the specific localized Dynamic System—see page 348); second there are the *specific distant* factors (activities in other members of the Dynamic System Family); and, finally, there are the *general* factors (activities in the brain tissue outside the loci of the various members of the Dynamic System Family). The pattern of activity in any one Dynamic System would be most sensitive to the specific local factors, less sensitive to the specific distant factors, and least sensitive to the general factors. However, all three would contribute to the final organization. This functional intercommunication among all Dynamic Systems provides for a *general* effect of brain activity upon all specific localized

Dynamic Systems—an effect that cannot be characterized as the mere summation of specific interferences.

There is an additional avenue for the operation of general effects within our theoretical model. I have stressed the fact that a Dynamic System organizes, in a unitary whole, every relevant neural event. Therefore any change in any single "type" of neural event could result in a change in the *total* organization of the Dynamic System. An injury, therefore, to any sensory projection area (which Goldstein labels the "periphery of the cortex") would reflect itself in a difference in the intrinsic organization of every Dynamic System since the contribution of visual stimulation, for example, to behavior is not to be regarded as a contribution to "visual behavior" only, but each visual stimulus is integrated in a *unified manner with all other kinds of stimulation*. In other words, a lesion in the visual cortex does not disturb the "visual function" only, but must disturb every Dynamic System which "uses" neural events originating in the visual cortex. This, again, would lead to a "general effect" (a change in *organization* of all Dynamic Systems) and not to a number of specific visual effects.

There is, finally, another possibility which we must take into account in discussing general brain function: the "physiological condition" of the brain. Perhaps the best way to indicate the role which this factor may play is to discuss, briefly, the "rigidity" of Dynamic Systems. The term "rigidity" is used here merely to refer to the speed or ease with which Dynamic Systems reorganize their activity patterns. We know that Dynamic Systems undergo reorganization but we can also assume that not all Dynamic Systems reorganize with the same speed or the same ease. What deter-

mines the speed with which Dynamic Systems can be reorganized? Obviously there are a number of such determinants, but let us consider only three of them—the three which, in my opinion, are the major ones. First, there is the nature of the starting state of the Dynamic System (*e.g.*, whether the Dynamic System is now in a stable state or not. In the former case reorganization would be more difficult than the latter). Second, there is the nature of the Dynamic System Family of which the Dynamic System in question is a member (*e.g.*, if the Dynamic System in question is a member of a large and stable Family then reorganization would probably proceed at a different rate than in the case where it is a member of a small Family, or of an unstable Family).⁶ Finally, I will assume that the physiological state of the tissue and fluids which make up the substrate of the specific Dynamic System can vary with respect to the ease with which they will permit a change in the redistribution of electrical-chemical activities (*e.g.*, an analogy might be the physiological state which determines the speed of neural impulses, or the permeability of nervous tissue, or the electrical conductance of the fluids, etc.). Such a postulated physiological variable could, most reasonably, be a general factor. A change in the general physiological condition of the entire brain (or, indeed, of the entire organism) could therefore show up as a change in the rigidity of all Systems. Presumably the behavior units coordinated with Dynamic Sys-

⁶ The relation between speed of reorganization and Dynamic System Family membership character is not a simple one. Under some conditions membership in a large family may result in increased ease of reorganization, under other conditions, in decreased ease of reorganization. For some analogues of what these conditions may be, see pages 135–138 of Krech and Crutchfield (12).

tems would reflect this "rigidity" characteristic of the Dynamic Systems. Thus we could expect to find some *general* behavioral characteristic which we could label "behavioral rigidity." This, of course, would not be in any conflict with our concept of unified and specific behavior units. Every unit could be described as varying along the continuum of behavior rigidity and yet we could, if we wished, refer to "the rigidity of the person," meaning by this latter phrase any one of a number of useful things. We could mean that all (or most) of the individual's Dynamic Systems are so organized as to make for difficult reorganization without reference to the physiological condition of the neuron system, or we could mean that the physiological condition of the individual's nervous system (whether hereditarily determined or not) is such as to make reorganization difficult, no matter what the state of his individual Dynamic Systems. In the first case we might be dealing with what we may term a "reactive rigidity," in the second with an "organic rigidity." Such a distinction, suggested by our present theoretical speculations, may prove to be a fruitful one in future studies of "rigid behavior," and perhaps merits a further word at this time.

"Reactive rigidity," in terms of the above analysis, would be a consequence of the specific state of the individual's present Dynamic Systems. The etiology of such a "rigidity" would, presumably, be sought for in the individual's psychological history. Further, we might expect to find that a subject with reactive rigidity was quite capable of non-rigid behavior in areas removed from those Dynamic Systems of his which were in a "rigid" state. On the other hand a case of "organic rigidity" would have a different etiology and would show rigidity in all (or al-

most all) situations—even in those situations which were isolated from his present Dynamic Systems. Furthermore, in a case of organic rigidity it is possible to conceive of an instance where the individual's Dynamic Systems tend to *remain in an unstable state*, for we must remember that the achievement of a stable state also involves reorganization. This suggests that the person who has organic rigidity may display some behavior units which are stable and can be *reorganized* only with difficulty, and also some behavior units which are unstable and can *achieve stability* only with difficulty. In other words, the extremely rigid person (in this sense of the term) may show both extreme order and structure in his behavior as well as extreme chaos and fluidity. This is, of course, exactly the observation which has been made by Frenkel-Brunswik (3), among others. Thus she writes: "... a rigid, cautious, segmentary approach goes with one that is disintegrated and chaotic, sometimes one and the same child manifesting both patterns in alternation or in all kinds of bizarre combinations."

A full discussion of rigidity of Dynamic Systems and behavior rigidity must be reserved for a later time. The only reason we have introduced this discussion now is to indicate how the assumption of variation in the general physiological condition of the substrate in which neural activity occurs can provide for "general" effects in behavior even though we postulate brain localization for the controlling Dynamic Systems. We have indicated, then, at least three different mechanisms⁷ whereby general factors can operate

⁷ These three are, to repeat, (1) Functional intercommunication among all Dynamic Systems, (2) common dependence upon the periphery of the cortex, and (3) a common physiological condition of the substrate.

within the specification set down for Dynamic Systems as *localized* events. The operation of these three mechanisms can be generalized by the formulation that *any behavior unit is determined by the events of the whole brain as reflected in the events occurring within the controlling Dynamic System or Systems*. This formulation gives the localized Dynamic System the "final" organizing or focusing function to play in the determination of behavior and, at the same time, makes possible the assimilation of all the experimental and clinical observations which speak for the importance of "brain action as a whole." Such a formulation also, it should be pointed out, permits the assumption of a determining role in behavior control by local *structural* changes, e.g., growth of synapse knobs, etc., and yet does not force us into the position of regarding the cortex as a "telephone switchboard." The above formulation can be seen as the theme sentence for my final specification of Dynamic Systems:

Dynamic Systems are restricted to localized brain areas. The nature and characteristics of these localized Dynamic Systems are determined by the events and conditions of the entire brain.

SUMMARY

Recognizing that Dynamic Systems, as hypothetical constructs, must be defined neurologically, the present paper has attempted to lay down three of the most general but pivotal specifications of Dynamic Systems. These three specifications are concerned with the *pattern* of activities in Dynamic Systems, the *open system* characteristics of Dynamic Systems, and the *locus* of Dynamic Systems. On the basis of various considerations the following attributes were postulated:

1. The Dynamic System is to be conceived of as a *field* of electrical-chemical activity. This field of activity, however, does not occur in a functionally or anatomically undifferentiated homogeneous matrix. The substratum for this field is articulated and can be described as consisting of subunits and primary loci of activity (neuron-chains, synapses, etc.) as well as intervening tissue which act as conductors. The neuron-chains and synapses can be thought of as the local organizing sub-structures of the entire activity field.
2. Dynamic Systems may develop spontaneously toward states of greater heterogeneity and complexity.
3. A Dynamic System, in its final reorganization, may maintain a steady state and continue to do work.
4. Dynamic Systems are restricted to localized brain areas. The nature and characteristics of these localized Dynamic Systems are determined by the events and conditions of the entire brain.

While this paper has restricted itself primarily to a discussion of the above attributes of Dynamic Systems as neurological events, at several points suggestions have been made concerning some possible behavioral implications of these attributes.

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SECONDARY REINFORCEMENT AS TERTIARY MOTIVATION: A REVISION OF HULL'S REVISION

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I. INTRODUCTION

Our problem is the nature of reinforcement. At the close of his admirable review of the law of effect Postman (25) asked three questions:

1. What is the agent responsible for reinforcement?
2. What is it that is reinforced?
3. What is the basic mechanism of reinforcement?

Since question 3 points to physiological obscurity, the present paper is concerned with questions 1 and 2; whatever progress we can make toward answering them should contribute to the ultimate solution of the third.

Before beginning we must take note of another attempt to answer Postman's questions. More boldly than the writer, Wolpe (41) aimed his solution directly at question 3. He defined *reinforcement* as the process of establishing functional neural connections; his proposal thus becomes, in intent, an inclusive theory of the neurophysiology of learning. For Wolpe the essential condition of learning is drive reduction. But since he defines *drive* as "central neural excitation," all stimuli are drive stimuli and drive reduction occurs whenever a stimulus ceases to act. *Needs* are a special class of excitatory condition chiefly characterized by the intensity of drive they produce, but their role in learning differs in no essential way from that of any other stimulus. They are important for learning simply because they are likely to have the strongest "drive-reduction potential" (41, p.

24). A single principle, according to its author, thus accounts for learning with and without need reduction and renders unnecessary the use of fractional goal reactions and secondary reinforcement by Hull and Spence to account for delayed reward learning. Since the theory to be outlined here stems largely from these concepts, it is imperative to consider the justice of Wolpe's claim.

We have seen that his explanation of how needs promote learning is based on the assumption that reinforcement is an increasing function of stimulus intensity. If this statement is completely general it should hold both within the class of needs and within the class of non-need stimuli. Evidence as to the relation between strength of need and learning is equivocal (5, 18, 21, 26). As to the relation between conditioned stimulus intensity and strength of conditioning, however, results of the few studies available (2, 6, 7) are clear and consistent. No relation between the two variables has been found.

A second assumption made by Wolpe is that reinforcement is a decreasing function of the time interval between synaptic activity and drive reduction. It follows that buzzer-shock conditioning should be less effective if the buzzer ceased at the onset of shock rather than at its termination. It was largely due to the implausibility of this inference, together with experimental evidence against it, that Mowrer (20) renounced the law of effect as a universal principle of learning. In view of these difficulties with Wolpe's challenging

thesis I feel justified in continuing the search for a more adequate solution.

II. A PROBLEM OF DEFINITION

Reinforcement is a term with many meanings. Wolpe made it coextensive with learning. Meehl (19), in his lucid exposition of the law of effect, confined it to one set of operations that leads to learning. A *reinforcer* Meehl defined as a stimulus change which, on being presented after a response, increases the strength of that response. *Reinforcement* is simply the presentation of a reinforcer. There is a third possible alternative that does not confine the term to the learning process. "Increase in response strength" does not necessarily mean increase in strength of *S-R* connections; it may mean simply that when a situation is repeated the response is more likely to occur. As we shall see, there are other ways than learning by which the probability of a response may be increased.

It is in this third sense that I believe the term reinforcement can be most useful at present. With this qualification in mind we may reword Meehl's definition as follows:

When a stimulus change *X*, following a response *R* to a situation *S*, increases the probability of *R* to *S*, *X* is called a *reinforcer* and its presentation is called a *reinforcement*. One purpose of the present article will be to develop the implications of this definition.

Hull gave the term a much more restricted meaning.¹ In his postulate 4 he specified two classes of reinforcing agent: (1) primary, "the diminu-

tion of a need"; and (2) secondary, "a stimulus which has been closely and consistently associated with the diminution of a need" (14, p. 178). Both kinds of reinforcer he made conditions of habit strength. Aside from the question of whether it is broad enough to cover all phases of learning, Hull's principle faces two difficulties: first, how to explain learning when reinforcement is delayed; secondly, how to explain the abrupt changes in performance that sometimes occur when the conditions of reinforcement are altered. Consideration of the first problem will, I hope, throw light on question 1 as to the nature of reinforcers. The second problem will provide a convenient approach to question 2 concerning the point at which reinforcement is applied.

III. WHAT IS THE REINFORCING AGENT?

Hull early recognized that much learning was too remote from any biological need reduction to fall under primary reinforcement. But even the concept of secondary reinforcement left two things to be accounted for: (1) Many learned acts precede their supposed points of reinforcement by appreciable time intervals. (2) The longer such time intervals are the more slowly performance improves. These facts and a number of related findings Hull dealt with by means of the "goal gradient," a function relating habit strength to the time between making a response and achieving a goal. He derived the goal gradient in turn from two factors: a primary gradient extending some 30 seconds backward from the point of

¹For this reason it will be impossible, in the following discussion, to adhere consistently to the usage just laid down. Context will have to determine meaning; within the

context of Hull's system *reinforcement* will carry the meaning he gave it; outside of that context it will carry the meaning specified above.

reinforcement, and secondary reinforcement acquired by stimuli falling within that gradient. The result was a composite curve made up of overlapping gradients. A basic assumption was that a reinforcing event could exert its effect on connections active as much as 30 seconds before. To some critics such an assumption was so implausible as to put the whole theory on a shaky foundation.

To meet that objection Spence (29) proposed an explanation of learning with delayed reward on the assumption that all reinforcement is immediate. He developed his theory especially to account for situations involving temporal delay, such as the T-maze studies of Wolfe (40) and Perkins (24) and the bar-pressing experiments of Perin (22, 23). Three principles are basic to his argument: secondary reinforcement, stimulus generalization, and perseveration of stimulus traces. Spence assumed, first of all, "that it is the particular stimulus pattern that occurs coincidentally with the food reward that acquires secondary reinforcing properties," and secondly, "that this conditioning generalizes to the stimulus patterns preceding it in time according to some gradient" (29, p. 5). Among the stimulus components mediating generalization Spence gave prominence to proprioceptive impulses from the critical response itself, since they frequently provide cues distinguishing a correct from an incorrect response, and since these cues diminish with time. As evidence Spence referred to experiments (8, 28) in which differential responses were eliminated and introduced and learning varied accordingly. A concrete example will illustrate the application of the three principles: In Perin's experiment (22), when a pellet of food appeared several seconds after the

rat pressed the bar, Spence assumed that proprioceptive traces of the response were still active and acquired secondary reinforcing power. This property generalized to the proprioceptive impulses set off by the next bar pressure, thus providing immediate secondary reinforcement of that response.

Details of this ingenious mechanism are not altogether clear. Stimulus generalization here involves two functions: increasing difference of stimulus pattern with time, and decreasing strength of secondary reinforcement as dependent on degree of difference. Spence left the forms of these functions unspecified save for the suggestion that they somehow combine to produce a negative growth curve. Yet his implication is clear: *The gradient of reinforcement is actually a special case of stimulus generalization gradient.*

Herein lies the chief significance of Spence's contribution. By questioning the primary status of the reinforcement gradient and showing how it might be derived from other principles he took a step toward logical economy within Hull's system. More pertinent to the problem at hand is the direction he gave our search for the reinforcing agent. Our attention is diverted from remote reward to the immediate context of the response itself, including its own sensory consequences, as the determining process.

The increasing emphasis being placed on secondary reinforcement, largely under Spence's influence, raises the question, "What does a secondary reinforcing stimulus do? How does it produce its effect?" Some years ago Hull suggested that primary and secondary reinforcement were basically the same. In his own words, "the . . . secondary reinforcing

stimulus acquires its power of reinforcement by virtue of having conditioned to it some fractional component of the need reduction process of the goal situation *whose occurrence, wherever it takes place, has a specific power of reinforcement in a degree proportionate to the intensity of that occurrence*" (14, p. 100). According to Hull the common factor in primary and secondary reinforcement is some part of the goal response, unconditioned in one case and conditioned in the other; this process, then, is the essential reinforcing agent.

By combining Hull's suggestion with Spence's we can now give a more detailed account of learning with delayed reward. This time we shall find it useful to take the case of a hungry rat in a simple T-maze with food in the right endbox. We assume, with Spence, that at the moment of finding and eating food there are proprioceptive consequences of turning right still reverberating in the rat's brain. They are thus conditioned to the consummatory response (R_G). When next the rat starts to make a right turn at the choice point, proprioceptive impulses are again aroused which, by stimulus generalization, evoke some fraction (rs_G) of the conditioned response to food.² Moreover, the pattern of impulses released by turning left is presumably more remote on the generalization gradient, thus evoking rs_G more weakly. We further assume, with Hull, that rs_G "reinforces" the response in progress in proportion to its own intensity, thus increasing the animal's tendency to turn right rather than left.

² The symbol rs_G indicates that the fractional goal response functions both as response and as stimulus. It may be thought of as a shortened form of Hull's $r_G \rightarrow s_G$, the goal reaction and its proprioceptive consequent, which would serve our present purpose just as well.

It may have struck the reader that this account of secondary reinforcement reads surprisingly like the development of an expectation. For Tolman a rat learns its way through a maze by forming sign-gestalts (recently renamed "field expectancies" [35]) consisting of three parts: "a sign, a significate, and a behavior-route leading from sign to significate" (32, p. 393). The two descriptions show obvious parallels: choice point stimuli with sign, rs_G with significate, and right-turning proprioception with behavior-route. In both cases the antedating goal reaction is made responsible for the greater strength of the appropriate response. In at least one vital respect, however, the two statements differ. According to the Hull-Spence version rs_G is primarily a habit builder; for Tolman it is rather a mobilizer of demand (31), cathexis (32), or "progression readiness" (34). That these two functions are not interchangeable will appear in the next section.

IV. WHAT IS IT THAT IS REINFORCED?

The question before us is how a reinforcer increases response strength. Does it do so by strengthening $S-R$ connections for future use, or does it help *on that future occasion* to bring about the response? In other words we are facing that much debated issue, is reinforcement a matter of learning or performance?

Field theorists have long held that learning consists primarily, not of the acquisition of $S-R$ connections, but of the organization of cognitions. As such it can, and indeed must, be distinguished from overt responding. Only thus can the role of motivation in behavior be understood. Motives, they hold (17, 38), regulate the *use* of habit, or better, knowledge, rather

than its acquisition. Performance is jointly determined by the demand for a certain goal object and the knowledge that it lies in a certain direction. But the demand and the knowledge cannot simply be added together, since if either factor is removed the response fails to appear. According to field theory the law of effect, dealing as it does with the influence of satisfiers, or goal objects, applies to motivation rather than cognition and thus to performance rather than learning (33).

The strongest experimental support for this view to date comes from the evidence for "latent learning"; e.g., the marked improvement in performance sometimes observed on the introduction of reward in an already familiar maze situation (1, 36). *S-R* theory, it is claimed, cannot give a satisfactory account of this finding because of the following basic defects, already implied above: (1) *S-R* theory treats learning as the forming and strengthening of receptor-effector bonds; (2) motives and habits are combined by algebraic summation; (3) in the absence of any clear-cut distinction between learning and either motivation or performance the law of effect becomes a principle of habit formation with no provision for sudden change.

As the most formal and explicit statement of *S-R* theory, Hull's system has borne the brunt of the critics' attack. To what extent are the above criticisms justified? With regard to the first there is little question that learning, for Hull, is a matter of strengthening receptor-effector connections. Concerning the second objection there is some doubt. When White (38) took Hull to task for adding the excitations from drives to those from external stimuli, Hull was able to reply that he had never definitely stated such a relationship

(39). Moreover, since the first mimeographed edition of his postulate set, dated February 12, 1940, he had made excitatory potential a function of the *product* of habit strength multiplied by the strength of primary drive. Yet in fairness to Hull's critics (38, 43) it should be recalled that he distinguished two factors in primary motivation, the drive (*D*) and the drive stimulus (*S_D*), and that he used only the first factor as a multiplier. Before 1940 the drive stimulus alone figured in his derivations. It functioned chiefly as a conditioned stimulus to successive responses (10) or as a mechanism for bringing forward the fractional goal reaction to the beginning of a behavior sequence (9) or for selecting one of a number of alternative behavior routes (11). These functions required habits involving *S_D* to combine with those involving external cues. As to *how* they combined, since 1940 Hull has repeatedly circulated a formula for the summation of habit strengths from different sources to the same response (13). In 1943 (14, p. 242ff.) he worked out a numerical example of the use of this formula in summing the habit-strength loadings of drive and non-drive stimulus components. If Hull had not definitely postulated the addition of such stimuli before 1940 he has since made up that deficiency.

As to the third criticism, after 1943 (14) it could no longer be said that Hull failed to distinguish between learning and performance; the two concepts were too clearly represented by his intervening variables of habit strength (*sH_R*) and reaction potential (*sE_R*), respectively. It was still true, however, as it had been since the publication of his goal gradient hypothesis (10), that for Hull the law of effect was a principle of habit formation. The concept of reinforcement,

in the sense of need reduction, entered directly into the formula for sH_R and only indirectly into that for sE_R . It will be important for our later discussion to examine this relationship in more detail.

Postulate 4 (14, p. 178) made habit strength a function, in part, of the number of occurrences of reinforcement together with its amount and its delay. For convenient reference the mathematical statement of the postulate is here reproduced:

$$sH_R = M(1 - e^{-kw})e^{-jt}e^{-u't'}(1 - e^{-iN}), \quad (1)$$

in which M is the maximum attainable habit strength, e is usually taken as 10, w measures the amount of reinforcing agent, t is its delay, t' is irrelevant to our purpose, N is the number of reinforcements, and k , j , and i are empirical constants.

It will be noted that the expressions for amount and delay of reinforcement enter the equation as multipliers and do not affect the value of the constant i . This is interpreted to mean that these factors determine the maximum habit strength for the conditions designated but not the relative increment per trial. Suppose two groups of rats are trained in a Skinner box under identical conditions except that group A receives an immediate reward of three pellets while group B receives one pellet after five seconds. By equation (1) the relative difference between the two groups will be the same at all stages of practice and will be carried over without distortion into the more inclusive formula for reaction potential (14, p. 242):

$$sE_R = f(sH_R) \times f(D). \quad (2)$$

But if at an advanced stage of training the conditions of reward are suddenly equalized for the two groups their

predicted reaction potentials will not immediately coincide. Hull has published the predicted results of an analogous case (14, p. 130), showing that the two curves will only gradually come together. This outcome becomes clear when we consider the equation for the increment of sH_R yielded by a single reinforcement:

$$\Delta sH_R = (M' - X)(1 - e^{-i}), \quad (3)$$

in which M' stands for the expression $M(1 - e^{-kw})e^{-jt}e^{-u't'}$ in equation (1) and X is the value of sH_R just before the reinforcement in question. A change of reward conditions changes the value of M' , but M' cannot be reached, or even approximated, in a single trial. Thus Hull's equations imply an "inertia" which until recently has prevented his system from handling certain data involving the manipulation of rewards (1, 4, 36, 42).

In November, 1949, however, Hull published a revision of his postulate set (15, 16) which opens up new possibilities. Since much of the ensuing argument will be based on this revision, it is assumed that the reader has a copy available. The parts relevant to our discussion are listed below, with the equations reproduced for more convenient reference³:

POSTULATE III. *Primary Reinforcement.*

Corollary i. *Secondary Motivation.*

Corollary ii. *Secondary Reinforcement.*

POSTULATE IV. *The Law of Habit Formation (sH_R).*

$$sH_R = 1 - 10^{-iN}. \quad (4)$$

POSTULATE VII. *Incentive Motivation (K).*

$$K = 1 - 10^{-k\sqrt{w}}. \quad (5)$$

³The letters chosen by Hull to denote constants are here changed to conform to equation (1) above.

POSTULATE VIII. *Delay in Reinforcement (J).*

$$J = 10^{-it} \quad (6)$$

POSTULATE IX. *The Constitution of Reaction Potential (sE_R).*

$$sE_R = D \times V \times K \times J \times sH_R \quad (7)$$

Substituting the right-hand expressions of equations (4) to (6) in (7) we have the following:

$$sE_R = DV(1 - 10^{-k\sqrt{w}})10^{-it}(1 - 10^{-iN}). \quad (8)$$

If we now compare equations (1) and (8) we see that there is a close resemblance between the expressions to the right of M and of DV , respectively. In fact, aside from the omission of the function for S - R asynchronism, $e^{-ut'}$, from the revision, the replacement of e by 10, and the substitution of \sqrt{w} for w , the two expressions are identical. We need only let $M = 1$ and then substitute the right-hand expression of (1) in (2) to make it seem that the computation of sE_R has been little changed by the revision.

Closer examination, however, shows that the apparent identity of the two sets of equations holds only as long as the amount and delay of reinforcement are held constant. For just as K and J no longer enter the equation for sH_R , neither do they necessarily enter the equation for ΔsH_R , the increment due to a single reinforcement. Thus if either factor is changed there is nothing in equation (8) to prevent it from exerting its effect directly and immediately on sE_R . In this respect J and K may be considered coordinate with D .

The implications of such an interpretation of equation (8) are quite far-reaching. In particular they seem to free Hull's system of its "inertia"

and enable it to encompass sudden shifts in level of performance even with D constant. A concrete example will make this point clear.

Suppose, as in Crespi's experiment (3), a group of rats is given one trial a day down a straight runway to food. For 20 days each rat finds four grams of food at the end of the runway. On the 21st day the amount is increased to 256 grams. Our task is to compute the reaction potential to running down the alley and, in particular, the amount of change to be expected from the 21st day to the 22nd.

To simplify the problem let us assume that $M = 1$ and $e^{-ut'} = 1$, and let us rewrite equation (1) to correspond with (8). Equation (1) then becomes:

$$sH_R = (1 - 10^{-k\sqrt{w}})10^{-it}(1 - 10^{-iN}). \quad (9)$$

We may also set D and V equal to 1, so that the right-hand expressions of (8) and (9) become identical. We shall further assign arbitrary values to the constants as follows: $k = 0.2$, $i = 0.03$, and, since we are not concerned at the moment with delay of reinforcement, $j = 0$. The computation of sE_R on the 21st trial (i.e., after 20 trials) is the same whether we use equation (8) or equations (9) and the simplest form of (2):

$$sE_R = (1 - 10^{-.2 \times 2})(1 - 10^{-.03 \times 20}) = .45.$$

But to compute sE_R on trial 22 by means of equations (9) and (2) we must add to the habit strength already accumulated with four grams of food reward the further increment due to one trial with 256 grams. Substituting in equation (3) we find:

$$\Delta sH_R = (1 - 10^{-.2 \times 16} - .45)(1 - 10^{-.03}) = .04,$$

which, added to .45, gives for trial 22:

$$sE_R = .49.$$

On the other hand, if we use equation (8) we are under no such requirement. To find sE_R on trial 22 we may simply substitute appropriate values in the same equation as on the preceding trial, thus:

$$sE_R = (1 - 10^{-.20 \times 16})(1 - 10^{-.08 \times 21}) = .76.$$

Moreover it is clear that this is exactly the equation which would be used if the rats had been rewarded with 256 grams from the beginning of practice. In other words, we have here a mathematical basis for predicting the type of result reported by Blodgett (1) and Tolman and Honzik (36) on introducing reward in the course of learning.

So much for the *mathematical* implications of Hull's revised postulates. If my interpretation is correct, his answer to the question at the opening of this section is not far removed from that of cognition theorists like Tolman. In so far as the conditions of reinforcement affect sE_R rather than sH_R , it is performance, rather than learning, that is "reinforced." When we turn to the *behavioral* implications of Hull's revision, however, certain difficulties appear that should be squarely faced.

In the first place, he has made habit strength dependent on the number of reinforcements but independent of their amount or delay. But it is a truism that anything in existence exists in some amount. It is inconceivable that a single reinforcement produces an increment of habit strength but that its amount is immaterial. It is conceivable that increases in need reduction *beyond a certain amount* have no effect. Re-

inforcements might even obey an all-or-none law like that of the transmission of nerve impulses. But if so there must be at least a minimum value, a kind of "threshold," below which reinforcement is without effect. Some such concept should then be included in the postulate set as a guide to its empirical determination.

The same reasoning applies to the time interval between a connection (sC_R) and its reinforcement. For one event to influence another they must somehow come together in time and space. Some form of contiguity is universally accepted as a prerequisite to learning. Therefore the delay of reinforcement must play some part in its effect on a modifiable connection, if only to set a limit beyond which it will have no effect at all. This conclusion may be derived from the argument for a threshold advanced above. Suppose sC_R continues its activity for some seconds in the central nervous system with a steadily diminishing intensity. The time will come when it will be too weak to be influenced by a reinforcing agent. Whether this relationship is all-or-none or a gradient, some relationship there must be. Hull's awareness of the fact is indicated by a footnote to his new postulate VIII in which he says: "It is probable that this postulate ultimately will be deduced from other postulates. . . . In that case the phenomena represented by J would be taken over in IX by sH_R . . . " (16, p. 4).

To summarize these comments, if reinforcement is to strengthen a connection it must accompany that connection with at least a minimum strength and within a maximum time interval. To omit these conditions is either an oversight or it implies that reinforcement is *not* a factor in learn-

ing and that the use of the term in the new postulate IV is gratuitous.

A second difficulty faced by Hull's revision is to explain how the amount and delay of reinforcement can contribute to sE_R independently of sH_R . If their effect is not incorporated in whatever neural structure or trace underlies sH_R , then they must be otherwise represented *at the time of making the response*. Since the amount of reward is often not present in the immediate surroundings and the delay of reward can never be, they must be supplied by the organism itself. But this simply means they must be learned, and the question remains, how? To account for latent learning it is not enough to free the incentive functions, J and K , from the construct of habit; we need a rational way of bringing them to bear on reaction potential. In the following section I should like to suggest one way in which this might be done.

V. THE NATURE OF SECONDARY REINFORCEMENT

As a first step we may profitably re-examine Hull's revised postulate III and its corollaries (16, p. 175). According to corollary i, secondary motivation arises "when neutral stimuli are repeatedly and consistently associated with the evocation of a . . . drive and *this drive* [italics mine] undergoes an abrupt diminution." By corollary ii, secondary reinforcing power is acquired when "a neutral receptor impulse . . . occurs repeatedly and consistently in close conjunction with a reinforcing state of affairs," which, by postulate III, is based on "the diminution in the receptor discharge characteristic of a need." These three excerpts considered together imply an intimate relation between secondary motivation and

secondary reinforcement. Both processes are produced by conditioning in the presence of a drive and its reduction. But conditioning of what? It will be recalled that in discussing Spence's theory of delayed reward learning we identified the secondary reinforcing agent as rs_G , the fractional antedating goal response. In the light of that discussion the following interpretation seems plausible: Secondary motivation, at least as regards appetites or positive cathexes, consists of a set to make a consummatory response (R_G).⁴ Secondary reinforcement, which may well comprise by far the largest part, if not all, of reinforcement, depends on some fractional component of R_G itself (rs_G). But if we think of a "set" as an incipient response, then secondary motivation turns out to involve an earlier phase of rs_G and secondary reinforcement a later one. To preserve the motivational character of both concepts without confusing them, we may call the later phase "tertiary motivation." We are thus led to a tertiary-motivation hypothesis of reinforcement: *a response is reinforced when it is motivated by rs_G and rs_G is facilitated.*⁵

⁴Hull has elsewhere recognized rs_G and its proprioceptive consequent sg as a source of motivation: "The rs_G , on the other hand, and so the sg , occurs persistently or at least intermittently throughout the behavior sequence. This persistence gives the sg certain dynamic powers of controlling action resembling the S_D which constitutes one aspect of motivation. The sg accordingly emerges as a kind of secondary motivational mechanism" (12, p. 7).

⁵Although the definition of tertiary motivation here proposed, as due to facilitation of a goal set, is confined to positive demands, a three-component theory of motivation is intended to apply to aversions as well. Thus pain may be considered a primary drive, fear a secondary drive, and anticipated fear reduction (or a fractional antedating escape reaction) a tertiary drive.

As a second step let us consider postulates IV, VII, and VIII of the revised set. In the light of the preceding paragraph all three functions are readily interpreted as factors contributing to the arousal of rs_G . K and J stand for the strength of the rs_G aroused; they state how large and how remote a satisfier the organism becomes set for. sH_R stands for the degree to which rs_G is conditioned; it states how rapidly that set is acquired. The three functions may therefore be combined, with K and J setting a limit to the positive growth function sH_R . They will then constitute a "law of tertiary motivation," which will define the symbol sH_{rs_G} by an expression identical with equation (9). This single principle takes the place of the three tributary postulates as well as corollary ii. At the same time it provides for the learning of the incentive functions J and K and thus supplies the deficiency earlier noted.⁶

As a third step we must deal with the response to be learned itself. Thorndike pointed out that in maze experiments "there is a mixture of learning to be able to go quickly to a certain place and of learning to wish to go there" (30, p. 458). Tolman, as we have seen, distinguishes between acquiring field expectancies and positive cathexes (35); the corresponding distinction in maze learning would be between field expectancies and "subgoal cathexes." A

⁶ It will be recalled that equation (9) was derived with slight modification from Hull's 1943 postulate 4. Essentially, therefore, my proposal amounts to restoring postulate 4 but re-interpreting it as a quantitative description of the acquiring of tertiary motivation. A similar suggestion was recently put forward by Reynolds (27), who proposed that Hull's 1943 statement of the relation between habit strength and amount of reward holds true only for "responses which can be considered replications of the final consummatory response" (p. 768).

similar distinction is conceivable between learning any response and acquiring a need to perform it.

The most economical solution, and the one here recommended, is to subsume "learning to wish" and "learning to do" under a single inclusive principle. For this purpose postulates III and IV may be combined in a simple, quantitative law of association (sH_R) omitting all reference to need diminution and reinforcement. Postulate VII may be similarly broadened into a general statement of the relation between stimulus intensity and response strength; postulate VIII, into a law of S-R asynchronism. In such a framework our "law of tertiary motivation" becomes merely a special case, though a highly important one, having to do with the learning of rs_G ; it therefore assumes the status of a corollary.

A fourth step is to combine the relevant factors in a single expression for reaction potential, as Hull has done in postulate IX. If the above proposal is accepted this expression will include three factors besides V : primary motivation (D), tertiary motivation (sH_{rs_G}), and habit strength (sH_R).⁷ Equation (7) will then be revised as follows:

$$sE_R = D \times V \times sH_{rs_G} \times sH_R. \quad (10)$$

What do these suggested revisions accomplish? In brief, they complete the formal separation between motivating and habit factors which Hull

⁷ The reader may wonder why secondary motivation is not explicitly represented in sE_R . In the case of hunger it is not clearly established that a set to eat can be aroused apart from primary drive, nor that it can differentially affect sE_R except by being facilitated, i.e., apart from tertiary drive. For the time being, therefore, we may omit it. In the case of specific cravings or appetites a factor of secondary motivation may well have to be included in calculating sE_R .

has partially recognized. At the same time they show how tertiary motivation and habit can both be derived from the same associative principles. They put the sole responsibility for reinforcement on sH_{rsG} and bring its influence directly to bear on sE_R .⁸ How this arrangement takes care of the phenomena of latent learning and of sudden changes in amount of reward will be dealt with at greater length in a sequel.

VI. SUMMARY

The term *reinforcement* refers to the strengthening of a response tendency by a subsequent stimulus. It has usually implied the strengthening of an S-R connection. Another possibility is that the reinforcer may somehow exert its influence on later occasions as an *acquired motive* to help activate the response in question. The present paper represents an exploratory step in this direction.

Our theory calls for an acquirable process to serve as a surrogate in the absence of the reinforcer. A likely candidate was found in the fractional antedating goal reaction (rsG). Here we made use of Hull's suggestion that secondary reinforcement depends on the conditioning of such a process, together with Spence's theory that delayed reward learning depends on the generalization of this conditioning to earlier trace segments.

Our theory must also provide a means whereby the reinforcing process activates the response. A precedent was found in Hull's recent revision of his postulates, in which he removed the functions representing

⁸ A possibility remains that sH_{rsG} may interact with sH_R in such a way as to alter its rate of growth. Such an effect, if demonstrated, would call for a further revision of postulate IX.

amount and delay of reward from the equation for sH_R and entered them directly in that for sE_R . But in doing so he neglected to put these factors *inside the organism*. To get them there the following further revisions were proposed:

1. All learning is to be based on a simple law of association, similar in form to Hull's original postulate 4 but without reference to reinforcement. Habit strength grows as a function of repetition to a limit set by response vigor and S-R asynchronism.

2. Conditioning of rsG is a special case of such learning limited by amount and delay of reward.

3. This conditioning results in a construct called *tertiary motivation* (sH_{rsG}), which enters the equation for sE_R as a multiplier along with D , V , and sH_R .

Our theory may be restated in the following nutshell: A reinforcer, in the case of positive cathexes, gains its potency through its ability, native or acquired, to arouse a consummatory response in the presence of the relevant need. In a learning situation this response becomes conditioned to stimuli associated with the preceding instrumental act. When these stimuli recur they therefore arouse tertiary motivation, which in turn facilitates the act in progress.

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REACTION LATENCY AS A FUNCTION OF REACTION POTENTIAL AND BEHAVIOR OSCILLATION¹

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It is a matter of common observation that reaction latency is a diminishing function of the number of times the act in question has been reinforced, and hence, presumably, of reaction potential. But no rational explanation of this relationship has yet been offered. Hull, for example, (1) has fitted an equation to latency data published by Simley, in which the latency is presented as the reciprocal of a fractional power of a positive growth function. This function fits the data very well, but there is no rational explanation of the fractional power.

Later on in his *Principles*, Hull has stated as a postulate (1, p. 344) that "the latency (sL_R) of a stimulus evoking a striated-muscle reaction is a negatively accelerated decreasing monotonic function of the momentary effective reaction potential ($s\dot{E}_R$), provided the latter exceeds the reaction threshold (sL_R)."¹ At first sight this postulate appears to be nothing more than a restatement of the common observation, but in fact it is not. It represents an attempt to explain sL_R by reference to an intervening variable, $s\dot{E}_R$, but this attempt raises more problems than it solves. Since $s\dot{E}_R$ varies from moment to moment it is difficult to understand what value of this function is directly responsible for the value of sL_R . Is it

the value that is operative at the moment when the stimulus is presented, or that which operates at the moment when the reaction occurs? It seems obvious that when sL_R is relatively long $s\dot{E}_R$ may take a number of widely different values in the interval of time between the presentation of the stimulus and the response. These considerations lead to the conclusion that sL_R is more likely to be determined by sE_R than by $s\dot{E}_R$. That is to say, latency is probably a function of the general level of reaction potential rather than of its momentary values.

This does not mean, however, that the moment to moment variation in reaction potential (sO_R) is irrelevant to the problem of reaction latency. On the contrary, it is probable that sO_R is an essential link in the causal chain which terminates in the reaction and determines its latency. But before proceeding to demonstrate the function of sO_R in this connection it will be necessary to amplify and clarify the conception of sE_R .

It may be accepted that under standard experimental conditions the general level of sE_R is an increasing exponential function of the number of reinforcements (n). But sE_R is something more than a mathematical function. It may be thought of as an active internal condition of the organism which comes into being in response to the stimulus, S , and *remains active* until the response R occurs, or until it is displaced by some other reaction tendency, $s_1E_{R_1}$. During the period of its activity sE_R is subject to reduc-

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tion by continuously varying amounts, sO_R , due to the random flux of internal and external conditions. For example, changes in the orientation of the animal to the stimulus result in changes in the pattern of afferent neural discharge initiated by S , and every such change in orientation involves changes in the pattern of afferent discharge aroused by other stimuli which, although they may be more or less irrelevant to the response that is about to be evoked, nevertheless contribute to the organism's total neural activity, and by afferent neural interaction modify the momentary strength of sE_R .

There is still some doubt as to the form of the distribution function of sO_R , and the functional relationship, if any, between its standard deviation (σ_O) and sE_R . For the purpose of the present discussion it will be assumed that the distribution of sO_R is Gaussian, and its standard deviation invariant. It will become apparent in the sequel that at least one of these assumptions is untenable, but they are adopted here because they facilitate the drawing of testable deductions.

Without making any assumptions about the form of the distribution of sO_R or about its standard deviation (σ_O), let us consider what happens when the sO_R distribution curve straddles the reaction threshold (sL_R). Let p be the proportion of the area of this curve that stands above sL_R , and $q = 1 - p$ the proportion that is below sL_R . Then p may be regarded as the probability that in any given unit of time $s\dot{E}_R > sL_R$. But this does not necessarily mean that p is the probability that a reaction will occur within that unit of time. Indeed there are excellent theoretical and empirical reasons for rejecting such an idea. For it is clear that the

unit of time must be at least as short as the minimum latency of which the experimental subjects are capable. It follows therefore that if p is the probability that a reaction will occur in any given unit of time, a proportion p of the subjects will react within the unit of time immediately following the presentation of the stimulus. That is to say, the decrease in average latency from trial to trial should be brought about chiefly by an increase in the proportion of animals that react with minimum latency. But this is not in agreement with empirical observation. In the first few trials not a single animal reacts with maximum speed, and the decrease in average latency is brought about by a general decrease in which all the animals share in varying degrees. We may conclude therefore that although an excess of $s\dot{E}_R$ over sL_R is a necessary condition for reaction evocation, it is not sufficient.

What further conditions have to be satisfied it is impossible to say in the present state of our knowledge, and for the moment all we can do is to speculate. We can, for example, appeal to the neurophysiological principle of summation of neural impulses, and assume that, especially in cases where the response involves a fairly rapid expenditure of energy, it is necessary to have either a number of impulses converging simultaneously on the relevant motor centers, or a number of impulses following each other at a sufficiently high frequency. But since the period of time over which temporal summation is effective is not more than a few milliseconds, whereas latencies may be many thousands of times as much, it would seem that temporal summation is scarcely relevant here.

It is known that an impulse trans-

mitted by a single fiber is in general insufficient to fire a neuron with which it is synaptically linked, and that when impulses from two fibers are delivered simultaneously to the same cell body a response is usually evoked. Now let us think of an external stimulus to which an animal is being conditioned as operating simultaneously on n cortical neurons or neuron assemblies; and let us think of p , the proportion of the oscillation curve above the reaction threshold, as defining the probability that any one of the n neurons will deliver an impulse to one of the motor neurons involved in the execution of the response within one unit of time after the presentation of the stimulus. But it is necessary that impulses from at least two of the n neurons should converge simultaneously on the motor neuron; and it follows therefore that the probability that the motor neuron will react within one unit of time after exposure to the stimulus is not p but p^2 . The probability that it will not react is $1 - p^2$.

For convenience let us put $i = p^2$, and $j = 1 - p^2$. Then the first term in the expansion of the binomial $(i + j)^k$ will represent the probability that k responses will be evoked in k units of time; the second term will give the probability that there will be $k - 1$ responses in the same period, and so on. That is to say, the $k + 1$ terms of the expansion give the several probabilities that the response will be evoked $k, k - 1, k - 2, \dots, 2, 1, 0$ times in k units of time.

If we multiply the binomial by a number representing the number of

experimental subjects in any investigation, then, assuming that the subjects are all identical with respect to the values of sE_R and sL_R and the constants of the sO_R distribution and therefore have all the same value of p , the terms of the binomial expansion as thus multiplied will give us the number of animals which may be expected to react $k, k - 1, k - 2, \dots, 2, 1, 0$ times in k units of time. This conception is of course meaningless when referred to empirical data, since in most experimental situations an animal, having reacted to a stimulus, is not immediately in a position to react to it again. However, we may turn the binomial expansion to account in the following way. The $(a + 1)$ th term of the expansion gives the probability that the conditions necessary for a response will arise $(k - a)$ times in k units of time. If we assume that these $(k - a)$ events are equally spaced over the k units of time, the interval of time between the presentation of the stimulus and the first of them being equal to the interval between any two successive occurrences, then the latency of the first event will be $\frac{k}{k - a}$. Hence the terms of the expansion of the binomial may be regarded as giving the frequencies of reactions whose latencies are:

$$1, \frac{k}{k-1}, \frac{k}{k-2}, \dots, \frac{k}{2}, k, \infty.$$

The abscissal scale of this frequency distribution is not linear, but the reciprocals of the abscissal values constitute a linear scale. Consequently any deductions we may draw from the binomial expansion may be regarded as meaningful if they are taken as referring to the reciprocals of the reaction latencies and not to the lat-

² In an earlier draft of this paper I had attempted to utilize the principle of temporal summation. I am indebted to Dr. J. Wolpe for pointing out my error and indicating how the principle of spatial summation might be applied.

encies themselves. For this reason it will be advisable to use a special symbol for the reciprocal of sR , and as this measures something analogous to velocity we may symbolize it by sV_R and call it Reaction Velocity.

We now proceed to deduce a number of corollaries from the characteristics of the binomial distribution, taken in conjunction with various postulates of behavior theory.

First we note that the mean of the binomial distribution is $A = ki$, and from this follows at once our first corollary.

COROLLARY 1

The mean sV_R is proportional to the square of the proportion of the area of the oscillation distribution curve that stands above the reaction threshold.

Since the upper limit of the distribution curve of sO_R is assumed to coincide with sE_R , it follows that as sE_R increases, the area of the curve that stands above sL_R increases, and this leads to our second corollary.

COROLLARY 2

The mean sV_R is an increasing function of sE_R , ranging from 0 when $sE_R \leq sL_R$ to k when $sE_R \geq sL_R + 6\sigma_0$.

Since the maximum value that i may take is unity, it follows that the maximum value of sV_R is equal to k . Hence the values of i in successive trials may be obtained by dividing the mean sV_R at each trial by the maximum sV_R . We may symbolize these quotients by:

$$V = \frac{\text{mean } sV_R}{\text{maximum } sV_R}$$

Then, from Corollaries 1 and 2 we derive:

COROLLARY 3

The square root of the ratio of the mean sV_R to the maximum sV_R (\sqrt{V}) is an increasing ogival function of sE_R , ranging from 0 when $sE_R \leq sL_R$ to 1 when $sE_R \geq sL_R + 6\sigma_0$.

We have assumed here that the effective range of the distribution of sO_R is 6σ .

From Corollary 2 we may deduce the 15th postulate of Hull's revised system of postulates (2). This postulate states that "reaction potential (sE_R) is a negatively accelerated decreasing function of the median reaction latency (sR).". If we plot sE_R against the reciprocal of sV_R we get a negatively accelerated falling curve, as the postulate states. It will not necessarily be precisely the same curve, since in the one case the abscissae represent the median sR , while in the other case they represent the sR corresponding to the mean sV_R . If the distribution of sV_R were always symmetrical the reciprocal of the mean sV_R would be equivalent to the median sR , but, as we shall see presently, the distribution of sV_R is not always symmetrical. Nevertheless the general tendency of the curve postulated by Hull is in agreement with our corollary.

It is well known that the binomial distribution is symmetrical only when $p = q$, or in our case when $i = j$. When $i < j$ the curve is positively skewed, and when $i > j$ it is negatively skewed. If, however, k is indefinitely increased, the index of asymmetry decreases, and even at extreme values of i the curve closely approximates to symmetry. For our present purpose, however, we may neglect this fact; for if k were indefinitely increased, then either the unit of time would be so small that no measurable changes

could take place in the organism within one unit and the unit would therefore be meaningless, or the period of time allowed for a single observation would be absurdly long. It may therefore be stated that if any deductions meaningful for behavior theory are to be drawn from the binomial theorem, k must be taken as being not large enough to obscure the asymmetry of the distribution when $i \neq j$. These considerations lead to:

COROLLARY 4

At the beginning of learning the distribution of sV_R is positively skewed. As learning proceeds the distribution approaches more nearly to symmetry and then gradually becomes more skewed in the negative direction.

Clearly this is a corollary that can be tested experimentally, but a word of caution is necessary. We have assumed that at any given stage in the learning process, that is, after a given number of reinforcements, the proportion of the sO_R curve that stands above sL_R is the same for all animals. In practice of course this ideal will not be achieved. The value of p will vary from animal to animal, so that the distribution of sV_R will not have the characteristics of a sample drawn from a homogeneous population but will be made up of elements taken at random from as many populations, having different means and different indices of asymmetry. Consequently the form of the distribution which theory leads us to expect will be distorted in practice. Its standard deviation is likely to be greater and its index of asymmetry smaller. From this we may deduce two further corollaries.

COROLLARY 5

At the beginning of learning the distribution of sV_R of a group of

animals selected on the basis of approximate equality of performance as measured by any valid criterion of learning is more skewed and has a smaller standard deviation than the distribution of sV_R derived from a random sample of animals.

COROLLARY 6

If animals are bred selectively on the basis of performance the distribution of sV_R at the beginning of learning will become more skewed and its standard deviation will become smaller with successive generations.

The standard deviation of the binomial distribution is given by:

$$\sigma = \sqrt{ki}$$

and the coefficient of variability is:

$$C = \frac{\sigma}{A} \times 100 = 100 \sqrt{\frac{j}{ki}}$$

and from this follow two further corollaries.

COROLLARY 7

As learning progresses the standard deviation of sV_R gradually increases, reaching a maximum when the distribution is symmetrical, and then gradually diminishes.

COROLLARY 8

The coefficient of variability of sV_R is a monotonically decreasing function of sE_R and hence of the number of reinforcements.

The proportion of the curve of sO_R that stands above sL_R is given by the probability integral of the sO_R distribution, from the ordinate which coincides with sL_R to infinity. It is desired to express this integral as a function of the number of reinforcements (n), in order to discover the functional relation of sV_R to n . We

begin by noting that:

$$sE_R = M(1 - F^a), \quad (1)$$

where F is a positive fraction, and M is the maximum sE_R , measured in units of σ_0 , which for the moment we shall assume to be invariant.

Assuming that the effective range of sO_R is 6σ , we note further that the range of sE_R with which we are concerned is from sL_R to $sL_R + 6$.

Let a be the number of reinforcements required to bring sE_R to the reaction threshold, and let b be the number of additional reinforcements required to increase sE_R to $sL_R + 6$. Then:

$$sL_R = M(1 - F^a) \quad (2)$$

and

$$sL_R + 6 = M(1 - F^{a+b}). \quad (3)$$

At both these values of sE_R the ordinate of the sO_R distribution curve coinciding with sL_R is virtually zero. In the former case this ordinate is at the positive end of the abscissal scale, in the latter case at the negative end.

The equation of the sO_R distribution may be written as:

$$y = f(x) = ce^{-\frac{x^2}{2}}, \quad (4)$$

where c is a constant and $x = sE_R - sL_R - 3$, measured in σ_0 units.

From (1) and (2) we have:

$$\begin{aligned} x &= M(1 - F^a) - M(1 - F^a) - 3 \\ &= M(F^a - F^a) - 3 \end{aligned} \quad (5)$$

substituting (5) in (4) we get:

$$y = \varphi(n) = ce^{-\frac{1}{2}[M(F^a - F^a)]^2}. \quad (6)$$

The effective range of x is taken to be from -3 to $+3$, that is, from $M(F^a - F^a) = 0$ to $M(F^a - F^{a+b}) = 6$. The maximum value of y occurs at $x = 0$, that is, at $M(F^a - F^a) = 3$. Since $1 > F > 0$, F^a is a negatively

accelerated decreasing function of n , and $M(F^a - F^a)$ is consequently a negatively accelerated increasing function of n . Hence the point on the n scale at which $M(F^a - F^a) = 3$ is nearer to a than to $a + b$. In other words, the curve of $y = \varphi(n)$ is positively skewed, and its ogive,

$$z = \int_{sL_R}^{\infty} \varphi(n)dn,$$

as compared with that of $y = f(x)$, is stretched more and more in the positive direction of the abscissal scale as it rises. These considerations lead to Corollary 9.

COROLLARY 9

The square root of the ratio of the mean sV_R to the maximum sV_R (\sqrt{V}) is a non-symmetrical increasing ogival function of the number of reinforcements (n), the upper half of the curve covering a wider abscissal range than the lower half.

We have hitherto assumed that the distribution of sO_R is Gaussian, and that σ_0 is invariant. We shall now investigate the consequences that will follow if either or both of these hypotheses should prove to be unjustified. There are a number of possibilities. σ_0 may be an increasing function of sE_R , or it may be a decreasing function of sE_R , or it may be an increasing function in one set of experimental conditions, a decreasing function in another set, and invariant in another. Or, finally, it may rise to a maximum at an intermediate level of sE_R and then diminish. Whichever of these possibilities turns out to be correct, there is one statement that we can make with reasonable certainty. At the moment when sL_R reaches its minimum value the whole of the sO_R distribution curve is

contained between the limits of the reaction threshold and the value of sE_R at that moment. From this follows Corollary 10.

COROLLARY 10

At the moment when sV_R reaches its maximum value:

$$\sigma_0 = \frac{M(F^a - F^{a+b})}{6}.$$

If the distribution of sO_R should prove to be other than Gaussian, the numerator in the above expression for σ_0 may be greater or less than 6.

If σ_0 is not invariant but is functionally related to sE_R , the ogival relationship between sV_R and sE_R will be modified. If σ_0 is an increasing function of sE_R , the curve of $sV_R = f(sE_R)$ will be progressively stretched in the positive direction of the abscissal scale as it rises. If σ_0 is a decreasing function of sE_R , the curve of sV_R will be progressively contracted as it rises. Finally, if σ_0 at first increases and then decreases, something closely approximating to a symmetrical ogive may be produced, although it will not be Gaussian. Corresponding effects will be produced on the curve of $sV_R = \varphi(n)$. This problem will be discussed in a further publication.

The need for a new theoretical approach to the problem of reaction latency arose out of a series of experiments designed to throw light on the question of behavior oscillation. In these experiments white rats were required to jump from a jumping platform on to either half of the divided entrance platform of a rectangular maze with two goal boxes, only one of which contained food. The latency of the jump was one of the measurements recorded. Initial attempts to fit equations to the latency measure-

ments failed, and the hypothesis presented above was developed.

But since the latency of any act of jumping might be determined by either of two reaction potentials of different magnitude, mediating jumps to the longer and shorter paths respectively, it was felt that this circumstance could obscure the relationship between reaction potential and reaction latency. Accordingly the experimental program was interrupted in order to perform an experiment designed to test some of the deductions from the new hypothesis.

The same rectangular maze was used, but the dividing wall at the entrance, separating the right-hand alley from the left, was removed, and 2 gm. of rat biscuit, moistened with water, was placed in each goal box. The element of choice, or discrimination, was thus eliminated, as well as any individual differences in performance that might arise from directional bias.

The subjects were 120 unselected albino rats, male and female, aged between 6 and 7 weeks at the beginning of the preliminary training. The procedure was that on the first day of preliminary training, 23 hours after its last meal, the rat was placed on the jumping platform, which was set near enough to the maze to allow the animal to walk over the gap. The animal was allowed to explore the maze, and when it reached either goal box and began to eat, it was allowed to eat for a few minutes and was then removed from the maze. An hour later it was given its daily ration of food. This procedure was repeated daily, but the gap between platform and maze was widened progressively to a maximum of six inches. If on any day the rat did not walk or jump over the gap within a few minutes, it was induced to do so by

TABLE 1

Group	#	Days of preliminary training	Duration of experiment
1	32	5	7
2	25	6	9
3	44	7	12
4	17	11	16

the experimenter, who picked it up, holding it so that its hind legs were in her hand but its front legs were in the air. From this position it generally escaped by jumping, and necessarily landed in the maze. On the first day when the rat jumped the six-inch gap without coaxing, the fact was recorded, together with the date. If it jumped again next day it was considered to be ready for the experiment, which for it began on the following day. The animals were thus divided into groups according to the number of days they required to reach the criterion of two successive unprompted jumps.

Of the 120 rats 118 had reached the criterion by the 11th day of preliminary training. The remaining two

were eliminated from the experiment. Table 1 shows, for each group, the number of animals in the group, the number of days of preliminary training, and the duration of the experiment in days.

On the day following the day on which any group reached the criterion the gap was, for that group, increased to eight inches, and it was kept at that distance throughout the experiment. No other change was made. A record was kept of various aspects of the rat's behavior on the jumping platform, including the jumping latency. We shall deal here only with the latency records. Other aspects of the platform behavior will be reported on by Mrs. Reichlin, who performed the experiment.

The reciprocals of the reaction latencies were recorded under the heading: Reaction Velocity (sV_R); and for each group the mean (A), standard deviation (σ) and coefficient of variability (C) of sV_R were computed for each day. The results are shown in Table 2.

It was hoped that by classifying the

TABLE 2

Day	Group 1			Group 2			Group 3			Group 4		
	A	σ	C	A	σ	C	A	σ	C	A	σ	C
1	.025	.026	104	.019	.020	105	.024	.023	96	.008	.007	88
2	.049	.053	108	.036	.040	111	.066	.055	83	.017	.017	100
3	.169	.139	82	.083	.103	124	.180	.194	108	.031	.029	94
4	.425	.261	61	.219	.168	77	.377	.397	105	.048	.038	79
5	1.148	.443	39	.438	.333	76	.614	.600	98	.085	.046	54
6	1.570	.347	22	.660	.506	77	.736	.557	76	.175	.117	67
7	1.850	.194	10	.988	.591	60	1.105	.620	56	.303	.233	77
8				1.284	.459	36	1.218	.588	48	.403	.255	63
9				1.724	.214	12	1.432	.494	35	.491	.272	55
10							1.600	.368	23	.579	.300	52
11							1.618	.382	24	.679	.263	39
12							1.768	.147	8	.844	.244	29
13										1.021	.235	23
14										1.194	.300	25
15										1.676	.226	13
16										1.735	.136	8

TABLE 3

Day	Group A (n = 33)				B (n = 28)				C (n = 39)				D
	A	σ	C	\sqrt{V}	A	σ	C	\sqrt{V}	A	σ	C	\sqrt{V}	
1	.031	.026	83	.124	.023	.022	97	.107	.016	.018	112	.090	.062
2	.064	.064	100	.179	.054	.049	90	.164	.030	.026	82	.123	.092
3	.275	.199	72	.371	.127	.112	88	.252	.060	.046	77	.173	.125
4	.629	.324	51	.560	.329	.201	61	.405	.129	.083	65	.254	.155
5	1.397	.438	31	.836	.725	.226	31	.602	.216	.116	54	.328	.206
6	1.822	.119	7	.955	1.040	.218	21	.722	.381	.231	61	.436	.296
7					1.643	.232	14	.908	.593	.279	47	.544	.390
8					1.779	.111	6	.944	.853	.301	35	.653	.450
9									1.310	.437	33	.810	.495
10									1.408	.391	28	.849	.538
11									1.458	.341	23	.855	.583
12									1.700	.153	9	.923	.650
13													.714
14													.770
15													.915
16													.931

animals on the basis of the number of days of preliminary training each group would prove to be homogeneous, and that it would be possible to test Corollary 5 by combining the distributions of two or more groups. This expectation has not been realized. Groups 1 and 4 are more nearly homogeneous than Groups 2 and 3. In all groups there are variations in the number of days required to bring the reaction velocity to its maximum value, but the range of this variation is less in Groups 1 and 4 than in Groups 2 and 3. In Group 3, for example, the minimum number of days is 6 and the maximum 12, whereas in Group 1 the minimum is 5 and the maximum 7, and in Group 4 the range is from 13 to 16. It was therefore decided to reclassify the animals on the basis of the number of days they took to reach a latency of .7 seconds or less. All animals, to whatever group they originally belonged, which reached this criterion in 5 or 6 days were classified in Group A. Those that took 7 or 8 days were put into Group B. Group C con-

tained those that took 9, 10, 11, or 12 days, and Group D those that took 13 to 16 days. Group D is identical with the original Group 4. One animal from Group 3 was excluded from the new classification. For the first few days its velocity increased at a pace equivalent to that of Group A animals, then decreased for a few days, so that it ultimately qualified for inclusion in Group C. Its effect on the constants of the sV_R distributions of Group C will be referred to below.

One further point of procedure must be mentioned. Group B includes some animals from Group 1, which was run for only 7 days. It was assumed that if they had been run another day their velocities would not have changed, and their velocities on the 7th day were therefore repeated in drawing up the distribution for the 8th day of Group B. Group C includes 15 animals from Group 2. For the last 3 days of Group C there was no record for these animals. It was felt that the repetition of their final velocities might have distorted

the distributions, and accordingly the constants of the sV_R distributions for Group C are based on 39 rats for the first 9 days and 24 for the last 3 days.

Table 3 presents the mean, standard deviation and coefficient of variability of sV_R for Groups A, B, and C. The values for Group D are identical with those given under Group 4 in Table 2. Table 3 also gives \sqrt{V} for all 4 groups. For this purpose the maximum velocity is taken to be 2, so that $V = \frac{A}{2}$.

Inspection of this table, together with the section of Table 2 which records results of Group 4, shows that for all groups the mean sV_R is an increasing function of the number of reinforcements and that the standard deviation first increases and then decreases. These findings are in agreement with Corollaries 3 and 7.

According to Corollary 8 the coefficient of variability should be a monotonically decreasing function of the number of reinforcements, and in Groups B and C this prediction is fulfilled. In Groups A and D there is an inversion near the beginning, but this does not necessarily disprove our hypothesis. One might argue that the difference between the first and second values of C is in both cases much smaller than the standard error of C and, therefore, cannot be regarded as significant. But such an argument would be entirely false. The standard error of a statistical constant indicates the extent to which the obtained value may deviate from the true value for the parent population in consequence of the sample not being truly representative of the population. But in the present case the first and second values of C are derived from the *same sample*, and the errors of estimate must be the same in direction and the same in amount

relative to the population values. In fact ordinary statistical considerations lead to the conclusion that the hypothesis cannot be sustained if the monotonic character of the function does not appear in every case, unless there is some rational explanation of the inversion, and this explanation is not in conflict with the hypothesis.

Table 2 shows that the original classification of the animals results in a more striking departure from the monotonic principle than the revised classification does, and this gives the key to the explanation of the inversions. The inclusion of animals with widely different rates of learning in one group means that after the first reinforcement some of the rats react with much higher velocity than the average of the group, and this has the effect that the standard deviation at first increases relatively more quickly than the mean. Then the slower rats begin to catch up with the quicker ones, and the coefficient of variability drops. When the animals are reclassified on the basis of their rate of learning, the distributions of sV_R are closer to the binomial distribution, and the monotonic tendency of the coefficient of variability is more likely to be realized. Even the revised classification, however, brings together animals that have different rates of learning, so that an occasional inversion in the curve of C is to be expected.

A striking confirmation of this explanation is afforded by the exclusion of a single animal from Group C referred to above. This animal at first increased its velocity at a rate comparable to the rats of Group A, and then reacted more slowly for a few days. When it is included in Group C the coefficients of variability for the first three trials are 112, 95, and 110; when it is excluded, the cor-

responding coefficients are 112, 86, and 77.

These facts confirm not only Corollary 7 but also that part of Corollary 5 which states that at the beginning of learning the standard deviation of sV_R is greater in a random sample of animals than in a sample selected on the basis of equality as determined by some valid criterion of learning. The addition of only one atypical animal to a relatively homogeneous group of 39 is sufficient to produce a significant increase in the coefficient of variability for the second and third days, and the inclusion of a few more animals of this type would increase it still further.

Here it should be noted that the design of the experiment was such that the reaction potentials were virtually the same for all animals on the first day, which would certainly not have been the case if they had all been given the same amount of preliminary training. The result is that when the mean and standard deviation of sV_R on the first day are calculated for the entire sample, the figures are not significantly different from those yielded by the groups. But on the second day, and still more on the third day, there are substantial differences in reaction potential within each group, and the standard deviation of sV_R (σ_V) is, therefore, relatively higher than on the first day. The reclassification of the animals has the effect of curtailing the extent of this relative expansion of σ_V , with the result that in Groups B and C the inversion of the coefficient of variability curve disappears.

These considerations suggest a revision of Corollary 8, which may be stated as follows:

COROLLARY 8a

If the experimental subjects are equal both in initial level of sE_R and in the rate of learning, the coefficient of variability of sV_R is a monotonically decreasing function of sE_R and hence of the number of reinforcements; if the subjects are equal in the initial level of sE_R but unequal in the rate of learning, the coefficient of variability may increase slightly before decreasing.

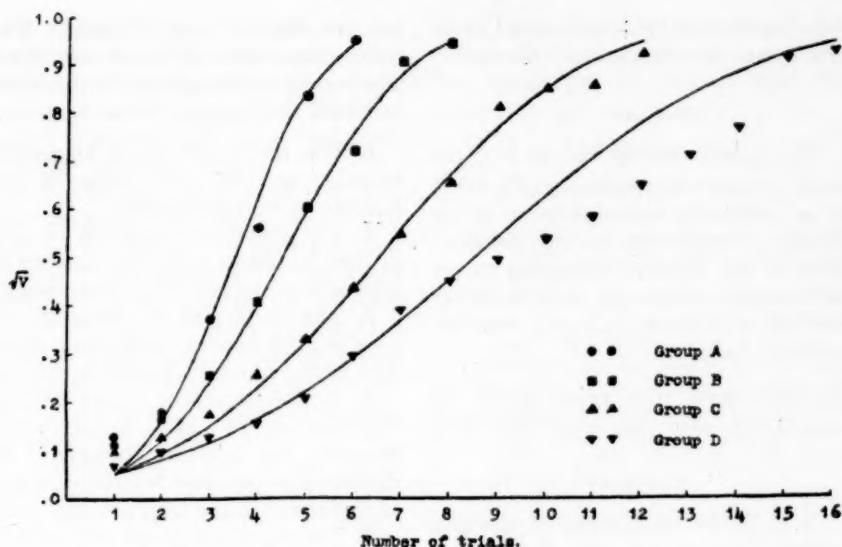
According to Corollary 4 the distribution of sV_R is positively skewed at the beginning of learning, then becomes symmetrical and gradually becomes skewed in the negative direction. The obvious way to test the validity of this deduction is to calculate the index of asymmetry of sV_R on the successive days of the experiment for each group. In each case it should begin with a high positive value and diminish through zero to a high negative value. But with such small samples it is difficult to determine the position of the mode with any degree of accuracy, and it would be absurd to calculate the index of asymmetry from the constants β_1 and β_2 in view of the large standard errors of these constants. Corollary 4 must therefore be tested by simple inspection of the frequency distributions. These are in general agreement with the prediction. There are, to be sure, some rather wide deviations from the expected curve forms, especially in the middle days of the experiment; but on the first and last days for each group the distributions are of the J form, being positively skewed on the first day and negatively on the last day. In the middle days the curves tend to be symmetrical, but very few of them approach the Gaussian form predicted by the theory. Consider, for example, the distribution for the 7th day of Group

C. The critical constants are: $\beta_1 = .03$, and $\beta_2 = 1.86$. That is to say, the curve is very nearly symmetrical, but it is markedly platykurtic. But again this cannot be taken as being incompatible with the hypothesis. Of the 39 animals in the group, 15 will reach the maximum velocity in two more days, while the remaining 24 will require 3, 4, or 5 days. It is to be expected that the quickest animals will augment the frequencies of the cells near the positive end of the scale, while the slowest animals will have a similar effect at the negative end. For an adequate test of the prediction it would be necessary to run several hundreds of animals, and to put into any one group only those that take exactly the same number of days to reach the criterion.

Finally, we may consider the deduction presented in Corollary 9, which states that the square root of the ratio of the mean sV_R to the maximum sV_R is a non-symmetrical increasing ogival function of the number of reinforcements, the upper half of the curve covering a wider abscissal range than the lower half. In Table 3 the last column for each group shows \sqrt{V} , and the final column in the table gives this value for Group D. It will be seen that in each case the form of this function is ogival, but the prediction that it would be stretched in the positive direction is not fulfilled. In the figure the values of \sqrt{V} are plotted against the number of reinforcements, and a Gaussian ogive has been drawn through each set of points. In the cases of Groups A, B, and C the Gaussian ogive fits the data reasonably well except at the beginning of the experiment. In the case of Group C the fit is good for the first 8 days and the last 2 days, but is very poor for the remaining

six days. There is a simple explanation of this deviation. The Group D animals, it will be remembered, were later than the other groups in reaching the criterion of 2 successive unprompted jumps. Reference to Table 1 shows that the experiment began for these animals on the day when Group 1 came to the end of the experiment, and when Group 3 had completed the experiment, Group 4 had still 8 days to run. By that time they were three months old and were sexually mature. Of the 17 animals in this group 12 were females, and it was observed that from the 9th day of the experiment onwards, they were much more restless than they had been during the earlier part of the experiment. They resisted capture, and on the jumping platform they displayed an increased tendency to exploratory activity. It may be assumed, therefore, that their deviation from the form of the curve shown by the other groups is due to the intrusion of the sex drive.

The prediction that the curve of \sqrt{V} would prove to be a non-symmetrical ogive was based on the assumptions that the distribution of sO_R is Gaussian, and that its standard deviation is invariant. These assumptions lead to the conclusion that if \sqrt{V} is plotted as a function of reaction potential it will be asymmetrical, and that the asymmetry will be even more marked if it is plotted as a function of the number of reinforcements. It is the latter function that is plotted in the figure, and as the curves here are very nearly symmetrical, it follows that one or more of the assumptions underlying Corollary 9 is false. It is beyond the scope of this paper to discuss in detail the significance of this finding, but it may be stated briefly that the



Square root of ratio of mean reaction velocity to maximum reaction velocity. The smooth curves are normal ogives.

data are consistent with the hypothesis that as sE_R increases the standard deviation of sO_R increases to a maximum and then decreases.

If this interpretation of the facts is correct, it would reconcile the apparently conflicting views of Hull and Taylor on the relation between σ_0 and sE_R . Hull (2, Postulate XIII, B) states that "The oscillation of sE_R begins with a dispersion of approximately zero at the absolute zero (Z) of sH_R , this at first rising as a positive growth function of the number of subthreshold reinforcements (N) to an unsteady maximum, after which it remains relatively constant though with increasing variability." Taylor (3) on the other hand, has shown that the results of one of Blodgett's experiments point to the probability that σ_0 is a decreasing function of sH_R . Hull's postulate is based on an experiment (4) in which a Skinner apparatus was used. The incentive motivation was presumably

substantially lower than in Blodgett's experiment, and consequently the asymptote of the sE_R curve was lower. If this asymptote is not higher than the value of sE_R at which σ_0 begins to decrease, then Hull's findings are not inconsistent with the hypothesis. In the case of the Blodgett experiment there is no evidence of an initial stage in which σ_0 increases, but this may be explained by supposing that the initial level of sH_R was not zero. This would obviously be the case if preliminary training had established a habit of reacting to any maze alley by running along it. In the experiment reported here preliminary training was continued only until reaction potential exceeded the reaction threshold, and the incentive motivation was substantial. It is, therefore, reasonable to suppose that the difference between the initial and final values of sE_R was greater than in either Hull's experiment or Blodgett's.

In the light of this discussion Corollary 9 may be restated as follows:

COROLLARY 9a

The square root of the ratio of the mean sV_R to the maximum sV_R (\sqrt{V}) is an increasing ogival function of the number of reinforcements (n), the exact form of this function depending on the relationship between the standard deviation of oscillation (σ_0) and reaction potential (sE_R).

In this form the corollary is in agreement with the empirical data.

SUMMARY

If p is the proportion of the distribution curve of sO_R that stands above the reaction threshold (sL_R), then p may be defined as the probability that in any moment of time $s\dot{E}_R > sL_R$. But this is not necessarily the probability that a reaction will occur. The neurophysiological principle of summation of neural impulses suggests that before a reaction can be evoked reaction potential must exceed the threshold in at least two successive units of time. The probability that a reaction will occur in a double unit of time is then $p^2 = i$, and the probability that it will not occur is $1 - p_2 = j$.

If we define a new term, Reaction Velocity (sV_R), as the reciprocal of reaction latency, it can be shown that the terms of the expansion of the binomial $(i + j)^k$ give the probabilities of the occurrence of reactions whose velocities are proportional to

$$1, \frac{k-1}{k}, \frac{k-2}{k} \dots \frac{2}{k}, \frac{1}{k}, 0.$$

From the properties of the binomial expansion, in conjunction with various postulates of behavior theory,

ten corollaries are deduced. The most important of these are summarized here, though not in the order in which they appear in the text.

1. The mean sV_R is an increasing function of sE_R and hence of the number of reinforcements.

2. The standard deviation of sV_R at first increases with the number of reinforcements and then diminishes.

3. The coefficient of variability of sV_R is a decreasing monotonic function of the number of reinforcements.

4. At the beginning of learning the distribution of sV_R is positively skewed. As learning proceeds the distribution becomes symmetrical and then skewed in the negative direction.

5. The square root of the ratio of the mean sV_R to the maximum sV_R is an increasing ogival function of the number of reinforcements.

These deductions are based on the assumption that the experimental subjects are homogeneous with respect to initial ability and rate of learning. The two following deductions take account of heterogeneity in these respects.

6. At the beginning of learning the distribution of sV_R is less skewed and its standard deviation is greater when the subjects differ in initial ability than when they are equal in initial ability.

7. If the experimental subjects are equal in initial ability but vary in the rate of learning, the standard deviation of sV_R increases relatively more rapidly than the mean, and the coefficient of variability increases slightly before beginning to decrease.

To test the validity of these deductions 118 white rats were trained to jump to a reward of 2 gm. of food. When each animal reached a criterion of two successive unprompted jumps

over a 6-inch gap, the gap was increased to 8 inches, and kept at this distance for the rest of the experiment. Jumping reaction latencies were measured and converted into reaction velocities.

The animals were classified into four groups in two ways: (a) on the basis of the number of days required to reach the criterion of two unprompted jumps, and (b) on the basis of the number of days required to reduce the latency to .7 seconds or less. By either classification the results for each group are in agreement with deductions 1, 2, 4, and 5 above. By classification b the results are in agreement with the 3rd deduction, by classification a, they are in agree-

ment with the 7th deduction. The design of the experiment precludes the possibility of testing the 6th deduction.

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